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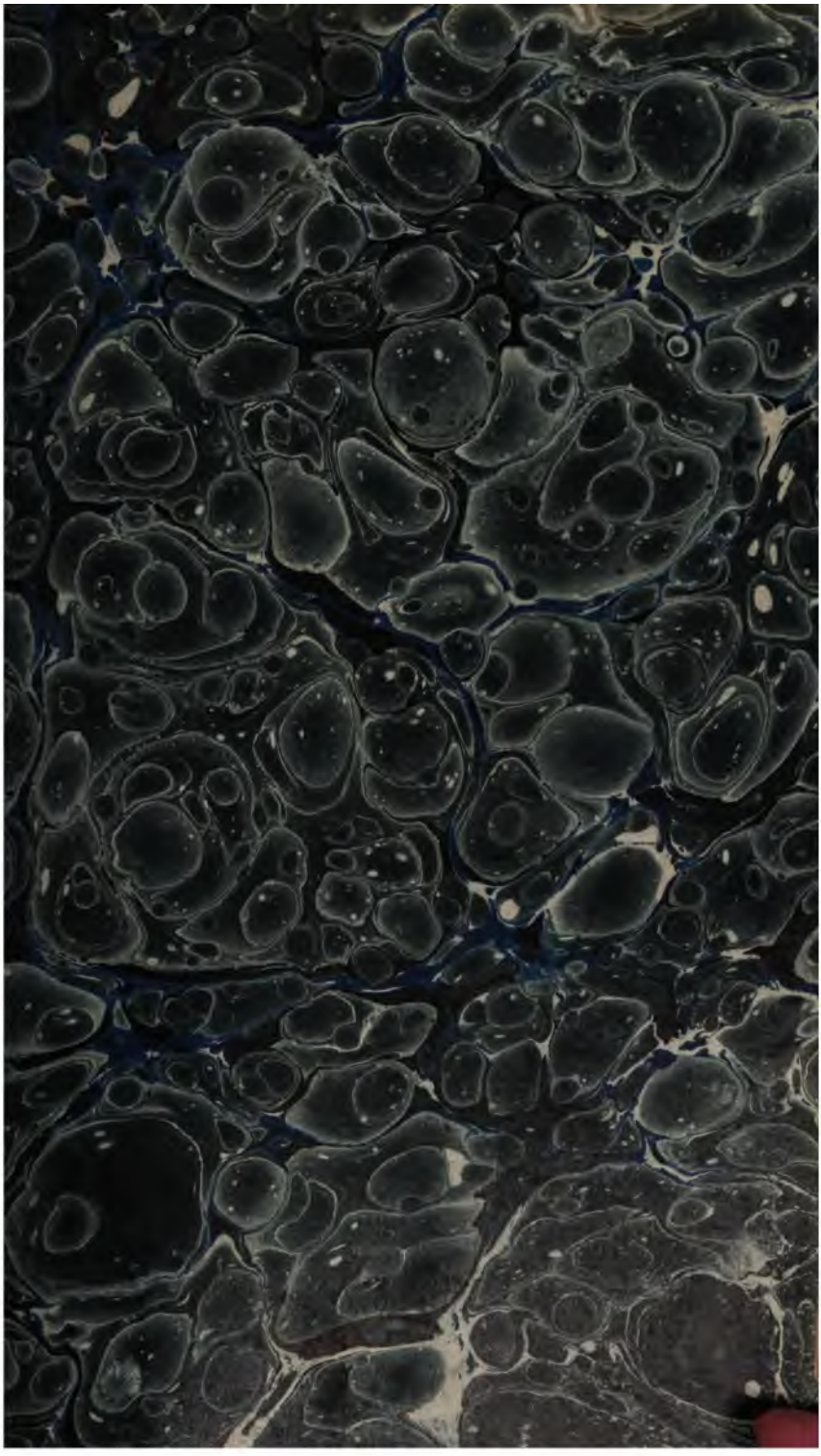

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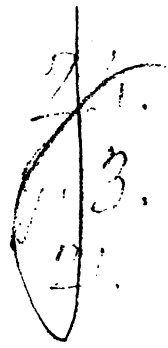

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# MEDICAL REPORTS,

ON THE

# EFFECTS OF WATER,

COLD AND WARM,

AS A REMEDY IN

## FEVER AND OTHER DISEASES,

*Whether applied to the Surface of the Body, or used Internally,*

VOL. I.

INCLUDING

AN INQUIRY INTO THE CIRCUMSTANCES THAT RENDER COLD DRINK, OR THE COLD BATH, DANGEROUS IN HEALTH.

TO WHICH ARE ADDED;

OBSERVATIONS ON THE NATURE OF FEVER;

AND ON THE EFFECTS OF

*Opium, Alcohol, and Inanition.*

THE FOURTH EDITION, CORRECTED AND ENLARGED.

By JAMES CURRIE, M. D. F. R. S.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS EDINBURGH.

*Intentiones operationum, quas proposuimus (ut arbitramur) veritissimæ sunt, remedia intentionibus fida. * * * Rem ipsam experimentum et comprobavit et promovebit. * * * Opera consilii cujusque prudentioris, sunt effectu admiranda, ordine quoque egregia, modis faciendi tanquam vulgaria.*

BACON. Historiæ vitæ et mortis,

*LONDON:*

PRINTED FOR T. CADELL AND W. DAVIES, STRAND; AND W. CREECH, EDINBURGH.

1805.

TO

FIELD MARSHAL

HIS ROYAL HIGHNESS

# THE DUKE OF YORK,

Commander in Chief, &c. &c. &c.

*SIR,*

*THE permission which I have received to dedicate this enlarged edition of these volumes to your Royal Highness, commands not merely my sincere gratitude, but my profound respect. They contain the details of a method of cure in febrile diseases in a great measure new, and which being particularly adapted to the medical practice of our armies, has been honoured with the notice of your Royal Highness. Under the sanction of your name and authority, this publication will obtain the general attention of our military practitioners, its merits will be fairly appreciated, and its usefulness, if established, be widely extended.*

*At a season when the profession of Arms acquires an importance unexampled perhaps in the annals*

*annals of mankind, whatever may contribute to the life and health of the defenders of their country, rises proportionably in value. In this point of view the present volumes are not, I hope, wholly unworthy of the patronage of one who is the Leader and Protector of British Soldiers.*

*That you may long enjoy these high distinctions, is the sincere wish of*

*Your Royal Highness's,*

*Most faithful*

*And obliged Servant,*

JAMES CURRIE.

Bath, July 7, 1805.

TO

TO THE

## RIGHT HON. SIR JOSEPH BANKS,

*Baronet, and Knight of the Bath, President of the Royal Society, &c. &c. &c.*

Liverpool, 31st October, 1797.

SIR,

IN presenting this volume to you,* I beg leave to say a few words on the views with which it was written, and the circumstances that gave rise to the publication.

About eighteen years ago, when I was at Edinburgh, it fell to my lot to write a paper on the influence of cold on the living body, for one of the societies of students, of which I was a member. In defending my speculations against some ingenious opponents, a perpetual contradiction occurred as to facts, which a reference to original authorities did not enable me to remove; for I discovered, that the accounts given of the temperature of the human body under disease, even by the most approved authors, are, with a few exceptions, founded, not on any exact measurement of heat, but on the sensations of the patient himself or his attendants.

* This Letter was prefixed to the first Edition.

Impressed

Impressed with the belief, that till more accurate information should be obtained respecting the actual temperature in different circumstances of health, and disease, no permanent theory of vital motion could be established, nor any certain progress made in the treatment of those diseases in which the temperature is diminished, or increased, I have occasionally since that time, observed and recorded such facts as related to the subject; intending, one day or other, to lay my observations before the public, if they acquired an importance that deserved attention. In the outset of this undertaking, nothing seemed wanting but accurate thermometers, and a moderate portion of time and attention; and I embraced in imagination the whole effects of temperature upon health and disease; a range of inquiry which experience has convinced me it would be temerity and folly to hope to go through. In this general view of the subject, some valuable communications have however been made to me by my much respected friend Dr. Percival; which do not apply to the particulars treated of in the following volumes, but which I am not without the hopes of specifying at some future period, when I may have occasion to employ them.

Though I have some time seen, that the delay of

of publishing till my original plan was executed, was likely to render my labours wholly abortive, or to convert the imperfect product from a gift into a legacy; yet I should not have given to the world so detached and unfinished a work as the present, according to the views I had originally entertained, but for the circumstances I am about to relate.

By the accounts received at the beginning of the present year, the fever of the West Indies appeared to continue its desolating progress with little abatement, and in America to be beginning its ravages anew.—With this pestilence, science seemed hitherto to have contended, in a great measure, in vain, and new methods of opposing it, were not merely justifiable, but requisite. At this time an account of the success of the nitric acid in Lues Venerea and Hepatitis, as employed by your correspondent Mr. Scott, of Bengal, was through your means, given to the public. His theory, suggested by the new chemistry, did not appear promising: but it was neither wise nor candid to reject his experience on that account. The first trials which I made of his practice persuaded me, that, though the success of the nitric acid might have been exaggerated by a warm imagination and a benevolent heart, it assuredly did succeed in certain cases of the diseases

eases in question, and that a remedy of *considerable power*, and *of perfect safety*, was introduced into medicine. These opinions subsequent experience has served to confirm. In the fever of the West Indies mercury had been much employed; though different notions were entertained of its mode of operation, it seemed on the whole the most approved remedy. Since the nitric acid appeared to be a substitute for mercury in other cases, it seemed reasonable to try its effects in this fatal fever. I suggested this practice by letter to some practitioners in the West Indies, and I submitted my notions on the subject to you; sensible that your character and station might bring that into immediate notice, which the influence of a private individual could but slowly effect.—Your conduct even exceeded my expectation.

Our correspondence on this occasion turned my views to the other means of opposing this pestilence. Ablution with cold water in fever had been so long employed at the hospital here, and in private practice, by my friends and colleagues Dr. Brandreth and Dr. Gerard, as well as myself, that it was become general in Liverpool, and common in the county of Lancaster. So long ago as the year 1791, a general statement by Dr. Brandreth of its advantages had been published

lished by Dr. Duncan, in the Medical Commentaries of that year. It had also been noticed by me in the Philosophical Transactions for 1792, and I had repeatedly mentioned it in private correspondence; it had often been recommended to the surgeons of African ships in those examinations required by the legislature, and which are chiefly made by the physicians and surgeons of our hospital. On different occasions likewise I had not only explained, but exhibited the practice, to practitioners from a distance, and particularly to one or two going to the West Indies. A method of treatment so bold, and so contrary to common prejudices, made, however, as it appears, slow progress. The mode of operation of our remedy has been misapprehended: the proper period for using it has not been understood; and on some occasions having been resorted to improperly, the consequences have brought it into disrepute. Reflecting on these circumstances, and exposed by situation to the reiterated sounds of death from the Western World, my decision was speedily made: I resolved no longer to delay an account of our treatment of fever, in the expectation of including it in a larger field of discussion, and of presenting it in a form more conducive to reputation; and the fruit of this determination is the work now presented to you. In treating my subjects, perspicuity has been studied rather

rather than rigorous method; I have every where endeavoured to make my steps so plain, that they may be distinctly traced; the most important points are impressed again and again, to guard against mistake; my thermometrical observations have enabled me to give a precision to the directions for the use of the effusion of cold water, which otherwise they could not have had; and, if I do not flatter myself, have laid a foundation for my reasonings, which speculations on fever have seldom possessed. I have guarded against the unnecessary use of technical as well as of general expressions. It were better perhaps that medicine, like other branches of natural knowledge, were brought from its hiding-place, and exhibited in the simplicity of science, and the nakedness of truth. If it had been in my choice, I would not have adopted the language of theory, like Boerhaave, or Sydenham; but have exhibited a medical work in the phraseology that Bacon, had he lived in our days, might have used. Unfortunately in the present state of medical knowledge, wholly to avoid the language of theory is impossible. The corruptions of false doctrines must remain more or less, in our phraseology, after the doctrines themselves are exploded; since custom has rendered the expressions in which they are found, intelligible, and human sagacity has not yet discovered those first principles of living

living motion, by which the doctrines and the language of physiology might at once be reformed. Hence the term *re-action* is applied to certain motions of life, though in a sense very different from that in which it is used in the science of inanimate motion from which it is borrowed; and such words as *tone* will still be found in the following pages, though the theory that introduced them into medicine be universally abandoned. The use of such expressions is however an evil, justified only by necessity; and I have endeavoured to avoid it as much as lay in my power.

Possibly this notice may procure me some readers among men of general science; and this I confess to be one of my objects in dedicating the work to you. It is naturally an author's wish that his book may be read by those who can appreciate it, and who from their situations may have it in their power to bring his precepts into practice. In both these points of view I appeal to you—to your scientific knowledge, and to your generous heart. The work that I address you is in a great measure practical. A man of genius, at the head of a fleet or army, would probably find little difficulty in understanding it; and possibly, if he understood it there might be occasions on which it would afford scope to his humanity and patriotism. But whatever be its fate with

with men professedly military, I trust it will not be overlooked by the medical practitioners of our fleets and armies; a most meritorious class of the profession, to whom a great part of the improvements in the modern practice of medicine is to be ascribed.

Conceiving that the circulation of this volume, as well as its uselessness, might be extended by connecting the history of the affusion of cold water in fever, with other views of the same remedy, and with a few observations on the other remedies in fever, I have entered on these points, without any very strict regard to method; and have been insensibly led to speak of some of the operations of temperature on the body in health, a subject which I have reserved. Such as it is, this volume may serve as the first of a series on similar subjects, if I should ever write them; and it may, I hope, stand alone, if I should write no more.

I am sensible that some of these particulars would have appeared with more propriety in a professed preface: but having entered on certain explanations in my address to you, I have given the whole of these preliminary observations in the same form; a freedom that I trust you will forgive.

I cannot

I cannot conclude without declaring the sense I entertain of your candour and politeness. Accept the tribute of my respect.—May you live long to cultivate and protect the sciences—the sciences, whose utility is beyond dispute; whose progress is superior to obstruction; and which, of all the possessions of man, seem least to partake of the imperfection of his nature!

I have the honour to be,

Sir,

Your faithful,

And very obedient Servant,

JAMES CURRIE.

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## *Advertisement to the third Edition.*

AN Apology is perhaps required for the delay which has occurred in presenting the third edition of this work to the Public, and especially to those Gentlemen by whose communications it is enriched.

The immediate pressure of my professional duties, and an imperfect state of health, have occasioned this delay. These circumstances must also plead my excuse for the work being printed with less accuracy than could be wished, and without the advantage of a new arrangement, which it was once my intention to have given it. But the copious Table of Contents and Index, will in some measure supply this defect.

J. C.

*May*, 1804.

# MEDICAL REPORTS, &c.

## CHAP. I.

### *Narrative of Dr. Wright.*

IN the London Medical Journal for the year 1786, Dr. William Wright,* formerly of the island of Jamaica, gave an account of the successful treatment of some cases of fever by the ablution of the patient with cold water.

"On the 1st of August, 1777," says Dr. Wright, "I embarked in a ship bound to Liverpool, and sailed the same evening from Montego Bay. The master told me he had hired several sailors on the same day we took our departure;



* Now President of the College of Physicians, Edinburgh, (1803).

one of whom had been at sick quarters on shore, and was now but in a convalescent state. On the 23d of August we were in the latitude of Bermudas, and had had a very heavy gale of wind for three days, when the above-mentioned man relapsed, and had a fever, with symptoms of the greatest malignity. I attended this person often, but could not prevail with him to be removed from a dark and confined situation, to a more airy and convenient part of the ship; and as he refused medicines, and even food, he died on the eighth day of his illness.

" By my attention to the sick man, I caught the contagion, and began to be indisposed on the 5th of September, and the following is a narrative of my case, extracted from notes daily marked down: I had been many years in Jamaica, but, except being somewhat relaxed by the climate, and fatigue of business, I ailed nothing when I embarked. This circumstance, however, might perhaps dispose me more readily to receive the infection.

" Sept. 5th, 6th, 7th, Small rigors now and then—a preternatural heat of the skin—a dull pain in the forehead—the pulse small and quick—a loss of appetite, but no sickness at the stomach—the tongue white and slimy—little or no thirst—

thirst—the belly regular—the urine pale and rather scanty—in the night restless, with starting and delirium.

" Sept. 8th, Every symptom aggravated, with pains in the loins and lower limbs, and stiffness in the thighs and hams.

" I took a gentle vomit on the second day of this illness, and next morning a decoction of tamarinds; at bed-time, an opiate, joined with antimonial wine; but this did not procure sleep, or open the pores of the skin. No inflammatory symptoms being present, a drachm of Peruvian bark was taken every hour for six hours successively, and now and then a glass of port wine, but with no apparent benefit. When upon deck, my pains were greatly mitigated, and the colder the air the better. This circumstance, and the failure of every means I had tried, encouraged me to put in practice on myself what I had often wished to try on others, in fevers similar to my own.

" Sept. 9th, Having given the necessary directions, about three o'clock in the afternoon I stripped off all my clothes, and threw a sea-cloak loosely about me till I got upon the deck, when the cloak also was laid aside: three buckets full of salt water were then thrown at once upon me;

the shock was great, but I felt immediate relief. The head-ach and other pains instantly abated, and a fine glow and diaphoresis succeeded. Towards evening, however, the same febrile symptoms threatened a return, and I had again recourse to the same method as before, with the same good effect. I now took food with an appetite, and for the first time had a sound night's rest.

" Sept. 10th, No fever, but a little uneasiness in the hams and thighs—used the cold bath twice.

" Sept. 11th, Every symptom vanished, but to prevent a relapse, I used the cold bath twice.

" Mr. Thomas Kirk, a young gentleman, passenger in the same ship, fell sick of a fever on the 9th of August. His symptoms were nearly similar to mine, and having taken some medicines without experiencing relief, he was desirous of trying the cold bath, which, with my approbation, he did on the 11th and 12th of September, and, by this method, was happily restored to health. He lives at this time (Jan. 1786) near Liverpool."

To this interesting narrative, Dr. Wright adds some general observations on the traces that are to be found of the use of cold water internally and externally in fevers, in several works ancient

cient and modern. But whether he himself pursued this practice any farther, I have not been informed.

Having before experienced that Dr. Wright was a safe guide,* I immediately on reading this narrative, determined on following his practice in the present instance; and before an opportunity occurred of carrying my intention into effect, I was farther encouraged, by learning, that my respectable colleague, Dr. Brandreth, had employed cold water externally in some recent cases of fever with happy effects.

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* See a paper in the Memoirs of the London Medical Society, vol. iii. p. 147, to be found in a subsequent part of this volume.

CHAP.

## CHAP. II.

*History of a fever which broke out in the Liverpool Infirmary.*

ON the 9th of Dec. 1787, a contagious fever made its appearance in the Liverpool Infirmary. For some time previously the weather had been extremely cold, and the discipline of the house, owing to causes which it is unnecessary to mention, had been much relaxed. The intensity of the cold prevented the necessary degree of ventilation, and the regulations for the preservation of cleanliness had been in some measure neglected. These circumstances operated particularly on one of the wards of the eastern wing, employed as a lock-hospital for females, where the contagion first appeared. The fever spread rapidly, and before its progress could be arrested, sixteen persons were affected, of which two died. Of these sixteen, eight were under my care. On this occasion I used for the first time the affusion of cold water, in the manner described by Dr. Wright.

It

It was first tried in two cases only, the one on the second, the other on the fourth day of fever. The effects corresponded exactly with those mentioned by him to have occurred in his own case; and thus encouraged, I employed the remedy in five other cases. It was repeated daily, and of these seven patients, the whole recovered.—In the eighth case, the affusion of cold water seemed too hazardous a practice, and it was not employed. The strength of this patient was much impaired by lues venerea, and at the time of catching the contagion, she laboured under ptyalism. I was not then aware that this last circumstance formed no objection against the cold affusion, and in a situation so critical, it was thought imprudent to use it. The usual remedies were directed for this patient, particularly bark, wine, and opium, but unsuccessfully; she died on the 16th day of her disease.

From this time forth, I have constantly wished to employ the affusion of cold water in every case of the low contagious fever, in which the strength was not already much exhausted; and I have preserved a register of a hundred and fifty-three cases, in which the cure was chiefly trusted to this remedy. Of these, ninety-four occurred in the hospital in the four years subsequent to the period already mentioned, twenty-seven in private

vate practice, and thirty-two in the 30th regiment of foot, when quartered in Liverpool in the year 1792. Of late (1797) I have not thought it necessary to register all the cases in which this remedy has been employed. Having satisfied myself of its extraordinary efficacy, and of the precautions necessary in using it, I have found it the shorter method, as well as the more instructive, to record the instances in which it has proved unsuccessful. To detail the whole of my experience would be a tedious and an useless labour. I purpose to digest the results under a few distinct heads, supporting and illustrating each general proposition by an ample detail of cases. Before, however, this preliminary account is closed, it will be useful to enter more particularly into the history of the contagious fever which broke out in the 30th regiment, because the account of its rise, progress, treatment, and termination, will support in a striking manner the doctrines I wish to establish, and, if I do not greatly deceive myself, may afford important instruction, as well as encouragement, to those whose duty may call them to oppose the progress of contagious fever in similar situations.

CHAP.

## CHAP. III.

*History of a fever which occurred in the 30th regiment.*

THE 30th regiment, as is usual with troops in Liverpool, was billetted in the town, but paraded and mounted guard in the fort, situated north of the town, on the banks of the river. The general guard-room had been used previous to the arrival of the 30th, as a place of confinement for deserters; it was extremely close and dirty, and under it was a cellar, which in the winter had been full of water. This water was now half evaporated, and from the surface issued offensive exhalations.

In a dark, narrow, and unventilated cell, off the guard-room, it was usual to confine such men as were sent to the guard for misbehaviour, and about the 20th of May 1792, several men had been shut up in this place on account of drunkenness, and suffered to remain there twenty-four hours, under the debility that succeeds intoxication.

tion. The typhus, or jail fever, made its appearance in two of these men about the first of June, and spread with great rapidity. Ten of the soldiers labouring under this epidemic, were received into the Liverpool Infirmary, and the wards allotted to fever could admit no more. The contagion continuing its progress, a temporary hospital was fitted up at the fort, and I was requested to give my assistance there to the surgeon of the regiment, by Captains Brereton* and Torriano.†

In two low rooms, each about fifteen feet square, were fourteen patients labouring under fever. They were in different stages of its progress: one was in the fourteenth day of the disease, two were in the twelfth, and the rest from the ninth to the fourth inclusive. The symptoms of the fever were very uniform. In every case there was more or less cough, with mucous expectoration: in all those who had sustained the disease eight days and upwards, there were petechiæ on the skin; in several there were occasional bleedings from the nostrils, and streaks of blood in the expectoration. The debility was considerable

* Now Colonel Brereton of the 63d foot.

† The gallant and accomplished officer who fell soon after at the heights of Pharon, in the defence of Toulon.

derable from the first, and it had been increased in several cases by the use of venesection, before the nature of the epidemic was understood. The pulse varied from 130 strokes in the minute to 100; the heat rose in one case to 105° of Fahrenheit, but was in general from 101° to 103°; and towards the latter stages of the disease it was scarcely above the temperature of health.—Great pain in the head, with stupor, pervaded the whole, and in several instances there occurred a considerable degree of the low delirium.

Our first care was to ventilate and clean the rooms, which were in a high degree foul and pestilential. Our second was to wash and clean the patients themselves. This was done by pouring sea-water, in the manner already described, over the naked bodies of those whose strength was not greatly reduced, and *whose heat was steadily above the temperature of health.* In those advanced in the fever, whose debility was of course great, we did not venture on this treatment, but contented ourselves with spunging the whole surface of the body with tepid vinegar, a practice, that in every stage of fever is salutary and refreshing.

Our next care was to stop the progress of the infection. With this view, the guard-house was at first attempted to be purified by washing and ventilation,

ventilation, the greatest part of its furniture having been burnt, or thrown into the sea. All our precautions and exertions of this kind were however found to be ineffectual. The weather was at this time wet, and extremely cold for the season; the men on guard could not be prevailed on to remain in the open air; and from passing the night in the infected guard-room, several of the privates of the successive reliefs, caught the infection, and fell ill on the 10th, 11th, and 12th of the month. In several of these the fever ran through its course; and in others, it was immediately arrested by the affusion of sea-water as already described. No means having been found effectual for the purification of the guard-room, it was shut up, and a temporary shed erected in its stead. Still the contagion proceeded; on the morning of the 13th three more having been added to the list of the infected. On that day, therefore, the whole regiment was drawn up at my request, and the men examined in their ranks: Seventeen were found with symptoms of fever upon them.—It was not difficult to distinguish them as they stood by their fellows. Their countenances were languid, their whole appearance dejected, and the tunica adnata of their eyes had a dull red suffusion. These men were carefully separated from the rest of the corps, and immediately subjected to the cold affusion, always repeated once, and sometimes

times twice a day.—In fifteen of the number the contagion was extinguished; but two went through the regular disease. On the same day, the commanding officer, at my desire, issued an order for the whole of the remaining part of the regiment to bathe in the sea; and for some time they were regularly mustered, and marched down at high water, to plunge into the tide.

These means were successful in arresting the epidemic: after the 13th of June no person was attacked by it. It extended to fifty-eight persons in all, of which thirty-two went through the regular course of the fever, and in twenty-six the disease seemed to be cut short by the cold affusion. Of thirty-two already mentioned, two died. Both of these were men whose constitutions were weakened by the climate of the West Indies; both of them had been bled in the early stages of the fever; and one of them being in the twelfth, the other in the fourteenth day of the disease when I first visited them, neither of them was subjected to the cold affusion. The water employed on this occasion was taken up from the river Mersey, close by the fort. It was at that time of a temperature from 58° to 60° of Fahrenheit, and it contains in solution from a 32d to a 33d part of sea-salt.

In

In hospitals, manufactories, and prisons—situations in which the low contagious fever so frequently originates, the practice I have detailed may be followed with great ease, safety, and advantage; but it is in a more particular degree applicable to this contagion when it appears on ship-board, because in that situation the usual means of prevention or cure are necessarily limited, and the imminence of the danger requires a remedy that operates with speed as well as efficacy. The waters of the ocean afford this remedy; in every point of view a most happy one for mariners, since it can be applied almost as easily as it can be procured.

CHAP.

## CHAP. IV.

*The manner in which the affusion of cold water ought to be used in fever.*

HAVING given this general account of my experience of this remedy in fever, it will now be necessary to enter more particularly on the rules which ought to govern its application, and on the different effects to be expected from it, according to the different stages of the disease in which it is employed. It will be proper to premise, that when the term fever is used in the present work without any adjunctive, it is the low contagious fever that is meant. This is the Typhus of Dr. Cullen; the contagious fever of Dr. Lind; the Febris inirritativa of Dr. Darwin. In popular language, it is generally called the nervous fever, and where particular symptoms appear, the putrid fever. It is usually produced in situations where there is a want of cleanliness, and more especially of ventilation; and when produced, it is propagated by contagion. This is the common fever of England;

land; its symptoms have been detailed with great minuteness in a variety of modern publications, and I have therefore declined repeating descriptions that are every where to be met with. Dr. Cullen has defined the disease as follows:—"Morbus "contagiosus; calor parum auctus; pulsus par- "vus, debilis, plerumque frequens; urina parum "mutata; sensorii functiones plurimum turbatæ; "vires multum imminutae." In sixteen years' practice I have found the contagious fever of Liverpool remarkably uniform, and in general to correspond exactly with this concise and perspicuous definition. This disease prevails chiefly among the poor, who from the nature of their diet and habits, are peculiarly exposed to the causes that produce it. Seldom extending itself in any considerable degree among the other classes of the community, it has been supposed that Liverpool was little subject to fever; but this will be shown from authentic documents, to be a great and a pernicious error. Let us proceed at present to inquire into the rules that ought to govern the use of the affusion or aspersion of cold water in this disease.

Whoever has watched the progress of fever, must have observed the justness of the observation made by Cullen, Vogel, De Haen, and others, that even those genera which are denominated continued,

tinued, are not strictly such, but have pretty regular and distinct exacerbations and remissions in each diurnal period. In this space of time, Dr. Cullen contends, that an attentive observer may commonly distinguish two separate paroxysms.* My observations do not enable me to confirm his position in its full extent—but one exacerbation, and one remission in the twenty-four hours, seem generally observable. The exacerbation usually occurs in the afternoon or evening, the remission towards morning. These exacerbations are marked by increased flushing, thirst, and restlessness. If the heat of the patient be, at such times, taken by the thermometer, it will be found to have risen one or two degrees in the central parts of the body above the average heat of the fever, and still more on the extremities.—The safest and most advantageous time for using the aspersion or affusion of cold water, is when the exacerbation is at its height, or immediately after its declination is begun; and this has led me almost always to direct it to be employed from six to nine in the evening; but it may be safely used at any time of the day, *when there is no sense of chilliness present, when the heat of the surface is steadily above what*



* Paroxysmis quovis die binis.

*Gen. Morbor. vol.* ii. *p.* 67.

*is natural, and when there is no general or profuse sensible perspiration.*—These particulars are of the utmost importance.

1. If the affusion of cold water on the surface of the body be used during the cold stage of the paroxysm of fever, the respiration is nearly suspended; the pulse becomes fluttering, feeble, and of an incalculable frequency; the surface and extremities become doubly cold and shrivelled, and the patient seems to struggle with the pangs of instant dissolution. I have no doubt, from what I have observed, that in such circumstances, the repeated affusion of a few buckets of cold water would extinguish life. This remedy should therefore never be used when any considerable sense of chilliness is present, even though the thermometer, applied to the trunk of the body, should indicate a degree of heat greater than usual.

2. Neither ought it to be used, when the heat, measured by the thermometer, is less than, or even only equal to the natural heat, though the patient should feel no degree of chilliness. This is sometimes the case towards the last stages of fever, when the powers of life are too weak to sustain so powerful a stimulus.

3. It is also necessary to abstain from the use of this

this remedy when the body is under profuse sensible perspiration, and this caution is more important in proportion to the continuance of this perspiration. In the commencement of sweating, especially if it has been brought on by violent exercise, the affusion of cold water on the naked body, or even immersion in the cold bath, may be hazarded with little risque, and sometimes may be resorted to with great benefit. After the sweating has continued some time and flowed freely, especially if the body has remained at rest, either the affusion or immersion is attended with danger, even though the heat of the body at the moment of using it be greater than natural.—Sweating is always a cooling process in itself, but in bed it is often prolonged by artificial means, and the body is prevented from cooling under it to the natural degree, by the load of heated clothes. When the heat has been thus artificially kept up, a practitioner, judging by the information of his thermometer only, may be led into error. In this situation, however, I have observed that the heat sinks rapidly on the exposure of the surface of the body even to the external air, and that the application of cold water, either by affusion or immersion, is accompanied by a loss of heat and a deficiency of re-action, which are altogether inconsistent with safety.—Each of these points will be illustrated more fully in the sequel.

Under these restrictions the cold affusion may be used at any period of fever; but its effects will be more salutary in proportion as it is used more early. When employed in the advanced stages of fever, where the heat is reduced and the debility great, some cordial should be given immediately after it, and the best is warm wine. The general effects of the cold affusion will be more clearly illustrated by the following cases. They are a selection from a great number, the records of which have been preserved, and which lead to the same results. They are so arranged as to exhibit the salutary effects of this remedy in the different stages of fever, and illustrate the precautions laid down against using it improperly. If they should appear tedious after what has already been mentioned, this must be forgiven; on a subject so important and so little understood, it is better to incur the charge of tediousness than the hazard of being obscure.

CHAP.

# CHAP. V.

## CASE I.

*Cases in which the Affusion of cold water was used in the different stages of fever.*

*January* 1, 1790.

A Nurse in the fever-ward of the Infirmary, having several patients under her care, caught the infection. She was seized with violent rigors, chilliness and wandering pains, succeeded by great heat, thirst, and head-ach. Sixteen hours after the first attack, her heat at the axilla was 103° of Fah^t^., her pulse 112 in the minute and strong; her thirst great, her tongue furred, and her skin dry.

Five gallons of salt water, of the temperature of 44° were poured over her naked body, at five o'clock in the afternoon, and after being hastily dried with towels, she was replaced in bed: when the

the agitation and sobbing had subsided, her pulse was found to beat at the rate of 96 strokes in the minute, and in half an hour afterwards it had fallen to 80. The heat was reduced to 98° by the affusion, and half an hour afterwards it remained stationary. The sense of heat and head-ach were gone, and the thirst nearly gone. Six hours afterwards she was found perfectly free of fever, but a good deal of debility remained.

Small doses of colombo were ordered for her, with a light nourishing diet, and for several days the cold affusion was repeated at the same hour of the day as at first; the fever never returned.

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During the progress of fever when epidemic, a great number of cases similar to the above have occurred, in which the disease was suddenly cut short by the use of the cold affusion on the first and second day; twenty-six of these cases were in the 30th regiment, as has been already stated. In such instances, the result was so precisely similar to what occurred in the case I have related, that it would be to no purpose to detail them.

When an epidemic fever is spreading, and the danger is known, patients will take the alarm on the

the first attack, and the power, as well as the utility of such a remedy as the cold affusion, in such situations of general danger, will be easily imagined.—It cannot be employed too soon after the first attack, provided the original chill is over, and the hot stage is firmly established.

In cases in which the affusion was not employed till the third day of fever, I have seen several instances of the same complete solution of the disease. I have even seen this take place when the remedy had been deferred till the fourth day; but this is not common.—The following case will point out the usual effects of this remedy in the third and fourth days of the disease.

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## CASE II.

*Jan.* 17, 1790. A. B. aged nineteen, a pupil of the Infirmary, caught the infection in attending the fever-ward. When I saw him, seventy-eight hours had elapsed since the first attack: he was of course in the fourth day of the disease. He had all the usual symptoms—head-ach, thirst, furred tongue, pain in the back and loins, with great debility. His heat was 101°, and his pulse 112 in the minute.

A bucket full of salt water was poured over him as usual, at noon on the 17th. His heat sunk to 99°, and his pulse to 98 in the minute. A profuse perspiration followed, with the cessation of all his feverish symptoms.—This intermission continued for several hours, during which he enjoyed some comfortable sleep: but at five in the afternoon he was again seized with feverish rigors, followed by heat, thirst, and head-ach as before. An hour afterwards, the hot stage was established; his heat was 100°, his pulse 100. The same quantity of cold water was again poured over him, and with similar effects. His pulse fell immediately to 80, and became more full; his heat became natural. The following night he took twenty drops of laudanum and slept well.

On the 18th at noon his pulse was 96 and soft; his skin moist, but a little above the natural heat. His tongue was a little furred, and his head ached: he also complained of thirst; the heat at the axilla was 100°.—The same remedy was again applied. He was greatly refreshed by it. The pulse fell to 90, the skin became cool, the thirst went off, and all the feverish symptoms vanished.

On the 19th his pulse was 88, his heat natural, the thirst and head-ach were gone, and his appetite

appetite improving. The affusion was repeated for the last time at six o'clock in the evening.

On the 20th his pulse was 78 and soft, his tongue clean, and his appetite farther improved. He had still some remains of debility on the 21st, but on the 22d he was free of complaint. This patient during his fever took no medicine but the effervescing mixture, the dose of laudanum excepted.—The affusion was used four times.

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## CASE III.

*Dec.* 8, 1791. A woman, aged fifty-seven, who had caught the infection in her attendance on a poor family labouring under fever, came under my care in the hospital, fifty hours after the first attack. She was of course in the third day of fever; she had the usual symptoms—head-ach, pain in the back and loins, and thirst: her tongue was furred and her eyes heavy: her pulse 96, her heat 101°. The affusion of cold water was performed at noon. In a few minutes afterwards the heat under the tongue was 98°, the pulse 80. Towards evening however, the feverish rigor, returned with all the usual consequences. As soon as the hot stage was established, the affusion was repeated, and with the usual happy effects.

*Dec.*

experienced great relief. The affusion was repeated on this and the following day—a gentle diaphoresis always succeeded it, with tranquil sleep, and on the third day the fever was completely removed.

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## CASE VI.

A young lady of 19, in the 7th or 8th day of typhus, became my patient, Sept. 26, 1794. Her pulse was 112 and feeble, heat 101°. She had great pain in the head, and much prostration of strength, her eyes were suffused and dull—her tongue furred—her spirits greatly depressed. Saline medicines were used for this patient, with lemonade for her usual drink, and moderate quantities of wine were given mixed with water. The burning sensation in the palms of her hands and temples was assuaged by frequent spunging with vinegar, and every evening at six P. M. three or four gallons of cold brine were thrown over her. The happy effects so frequently described, were in this case particularly striking:—The pulse fell almost immediately to 90, the heat to its natural standard, and the head-ach vanished—a gentle diaphoresis followed, with easy sleep:—in a few hours, however, the feverish symptoms returned, and towards the hour of six in the evening, the fever was in its highest state of exacerbation. At this hour therefore

fore the affusion was repeated with the same happy effects—though the fever returned as before, it was in a milder form; the same practice was continued, and on the second of October, she was entirely free from the disease.

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## CASE VII.

F. G. a soldier of the 30th regiment, aged 33, fell under my care on the 9th of June, 1792, during the prevalence of the epidemic in that regiment, of which I have already given an account. He was in the 9th day of the disease—his pulse 100 and feeble—his heat 104°—his thirst was very great—his tongue foul and black—frequent cough occurred, with streaks of blood in the expectoration—and petechiæ appeared all over his body. His mind was at all times confused, and at times he was completely delirious. I directed that his strength should be supported by administering a bottle of wine every day, with an equal quantity of gruel;—that every night he should take an opiate draught, and that a complete operation of his bowels should be procured by a clyster administered daily, and if this did not succeed, by a few grains of calomel. I also directed that a bucket full of salt-water should be thrown over him immediately, and repeated according to circumstances. In a few minutes after the affusion, the

the heat was 98°—the pulse 98—his mind was more calm and collected: two hours afterwards he had relapsed into nearly his former state, but the night was passed more tranquilly. The whole of this practice was continued with nearly the same result, till the 12th day of the disease, the affusion having always been performed in the evening, and sometimes at noon also. The fever continued its usual period, but on the 12th day, the heat having sunk to its natural standard, the cold affusion was thenceforth omitted; we however, spunged the whole body once or twice a day with vinegar. —The patient was in a state of convalescence on the 18th day from the first attack.

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I have related this case the more circumstantially, because it contains the particulars of my practice in the epidemic in which it occurred, at the same time that it affords an example of the effects of the cold affusion used in the more advanced periods of fever. In such instances, as might be expected, it does not procure the same advantages as in the earlier stages, when the strength is less impaired, and the morbid actions less firmly associated; nevertheless, it is evidently advantageous while the heat of the patient exceeds the natural standard, though it ought to be employed with caution in the more advanced stages of

of the disease, and in such cases, according to my later experience, of a temperature from fifteen to twenty degrees only, below the human heat. In the greater part of the cases that I have related, the water employed was the pump-water of our hospital, saturated with sea-salt, and of a temperature from 40° to 50 of Fah$^{t}$.

The cold affusion may also be employed with success in intermittent fevers, as I have found by repeated trials, and as the following case will demonstrate.

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## CASE VIII.

Ann Hall, aged 22, was admitted into the Infirmary, July 19, 1792, under an obstinate quotidian of three months' standing—she had from time to time taken the bark, but as the great delicacy of her stomach, would not permit her to use it in sufficient quantities, she was become very feeble and much emaciated. A gentle emetic was administered to her in the first instance, and on the commencement of the hot stage of the paroxysm, twenty drops of the tincture of opium were directed to be given to her, after the practice of Dr. Lind. During the intermission, the bark was ordered to be taken in such doses as her stomach would bear; she was put on a nourishing diet, and

was

was ordered a pint of port wine every day. This plan was pursued for fourteen days, but without success; the paroxysms returned daily, though with some irregularity—her strength was however rather improved. Still the delicacy of her stomach continued, and the bark, except in very small doses, was constantly rejected.

On the 8th of August, two hours before the expected accession of the fever, four gallons of brine were dashed over her, of the temperature of 66° of Fah$^{t}$., and this day she escaped the attack. In the interval between this and the period of the next return, she took the bark in larger quantity, the power of her stomach being increased; but on the 10th, two hours later than usual, the paroxysm returned with unusual severity. *After the hot stage was completely formed*, the brine was poured over her, as before—the symptoms instantly abated; she fell into a gentle perspiration, with profound sleep. She afterwards continued the bark as before, and from this time forwards was free of disease.

It would be easy to multiply these details, but their uniformity has already perhaps rendered them tedious; a few general observations shall therefore conclude this division of the subject.

CHAP.

# CHAP. VI.

## *General Observations.*

1. THOUGH the patients were often startled at the first proposal of dashing the cold water over them, yet, after one trial, there was seldom any difficulty in persuading them to have it repeated. The effects were in general highly grateful and refreshing to their sensations; the extinction or abatement of fever was commonly followed by more or less diaphoresis, and this again by refreshing sleep.

2. At first I used fresh water—afterwards fresh water mixed with vinegar—and lastly, a saturated solution of sea-salt in water. In the instance of the 30th regiment, I used the water of the river, which contains about a thirty-third part of salt, as has been already mentioned, and this I commonly

employ in private practice.* I was led to prefer salt water to fresh on account of the stimulating effect of sea-salt on the vessels of the skin, by which I apprehend the debilitating action of cold is prevented. Salt water, either for the purpose of immersion or affusion, is more grateful to the patient than fresh water, and it is well known that it may be applied to the surface for a length of time, with much less hazard. Persons immersed in sea water, and especially in saturated brine, for some time together, preserve the lustre of the eye and the ruddiness of the cheek, longer than those in fresh water, of an equal temperature, and such persons exhibit the vital re-action stronger when

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* It may be supposed, that the degree of impregnation of the river water with salt must depend on the time of the tide when it is taken up; thus, that it must be salter at the height of the flood, when the tide has run six hours from the sea, than at the period of low water, when it has run nearly the same time from the land.—I expected to find this the case, and endeavoured to ascertain the difference; but in a trial which I made by evaporating 40lbs. of water taken up at the height of the flood tide, and the same quantity taken up at low water, I could perceive no difference in the proportion of salt, a circumstance which I am unable to explain. The small difference that there was in the residuum in favour of the water taken up at the top of the tide, arose evidently from its being mingled with a large portion of dirt—the water at the top of the tide is mixed with dirt and feculencies, that taken at the lowest point of ebb is nearly pure.

when removed from it. I preferred the brine to vinegar, as being cheaper, and more easily procured of the necessary quantity: otherwise, it is well known how grateful vinegar is to patients in fever, and perhaps a mixture of vinegar and water of the proper strength, might be preferable even to brine. But though I gave the preference to brine over fresh water, I have very often used the latter, and it is seldom that any danger can result from the want of a saline impregnation, where the cold is employed in so stimulating a form as that which has been described; that is, suddenly, and for so temporary a duration.

3. In taking the heat of the patient, I have generally used a small mercurial thermometer of great sensibility, with a moveable scale, made for me by Mr. Ramsden, after a form invented by the late Mr. Hunter, and used by him in his experiments on the heat of animals, and I have introduced the bulb under the tongue with the lips close, or under the axilla, indifferently; having found by repeated experiments, that the heat in these two places corresponds exactly, and gives a just indication of the heat of the surface of the body, where sheltered by the necessary teguments from the contact of the external air.

Finding, however, considerable risque in using

the straight tubed thermometer in contagious diseases, I got some instruments of this kind made with a small bulb and curved at the end. The bulb being introduced under the tongue or the axilla, the observer can stand behind the patient, and mark the rise of the mercury, without coming into the immediate sphere of his respiration. Though no injury was in any case incurred from the use of this thermometer, yet a farther improvement has suggested itself. By introducing a small piece of iron into the tube, after the manner of Mr. Six, a permanent indication of the greatest heat is obtained, and the approach of the observer towards the patient during the experiment, is rendered unnecessary.

CHAP.

## CHAP. VII.

*Precautions requisite in using the cold affusion, illustrated by Cases.*

1. IT was before remarked that the cold affusion cannot be used with safety during the cold stage of the febrile paroxysm: the following case will illustrate this truth. In the summer of 1792, I was requested by Mr. Hoffman, an ingenious Prussian gentleman, and a surgeon in the army then under the command of the Duke of Brunswick, to give him an opportunity of seeing the method of using this remedy. At that time there was a patient labouring under a tertian intermittent under my care in the infirmary, on whom it could with propriety be exhibited. Accordingly a time for meeting Mr. Hoffman in the fever-ward was appointed, when the hot stage of the paroxysm might be expected to be fairly formed. It happened however that the accession of the fever had occurred an hour later this day than might have been expected,

ed, and when we arrived, the patient was still in the cold stage of the paroxysm; the assistants however proceeded: he was taken out of his bed shivering, his pulse small and frequent, his extremities shrunk and cold. In this state the brine was dashed over him as usual, but not with the usual happy effects;—his breathing was for some minutes almost suspended; his pulse at the wrist was not to be felt; the pulsations of the heart were feeble and fluttering; a deadly coldness spread over the surface; and when respiration returned, it was short, irregular, and laborious.—After the use of frictions on the surface, and particularly on the extremities—of a steady warmth applied for some time to the scrobiculus cordis—and of cordials cautiously administered in small quantities—the pulse at the wrist returned; but for some time it was excessively quick and feeble. He recovered however in the course of an hour, and it was found that the paroxysm of fever had been extinguished: but the circumstances first related, were evidently full of danger, and they produced at the time much apprehension and uneasiness. The same remedy was however used in the hot stage of the ensuing paroxysm, and with the usual happy effects.—Other cases to the same purpose might be adduced if it were necessary.

I have frequently used the cold affusion in the hot

hot stage of the paroxysm of intermittents, and almost always with the immediate solution of the fit; but in general, if no remedy be used in the intermission, the fever returns at the usual period. In some instances, however, the succeeding paroxysm has been prevented by using the cold affusion about an hour previous to the period of its expected return, and the disease ultimately removed by continuing this practice through four or five of the following periods.

The use of the cold affusion in the absence of fever, requires however a constitution in a great measure unbroken; and many of the intermittents which we see in Liverpool, being transmitted to us from the warm climates, adhere to constitutions in which this practice is not perfectly safe. In such cases, it may notwithstanding be adopted in the hot stage of fever with safety and advantage. Indeed it ought never to be forgotten, that an application of cold, which is safe in the violence of fever, is not safe when the fever is removed. Injury has sometimes occurred from continuing the cold affusion in the period of convalescence.

2. Neither is the cold affusion safe after the sweating stage of fever has continued some time, and the body is passing through that cooling process. The following case will illustrate this position.

tion. In the summer of 1791, a boy of eight years of age, in whom I am peculiarly interested, was attacked by fever. On the third day his pulse rose to 130, and 140 in the minute, and his heat to 106° and 107° of Fah'. His thirst was very great, and delirium commenced on the second day, and continued without intermission. Various methods had been employed to abate the fever, and particularly to excite sensible perspiration, but unsuccessfully. His heat was not lessened by repeated spunging of the surface of the body with cold vinegar and water; and after a copious bleeding, all the symptoms were as alarming as before. It seemed hazardous to repeat this evacuation, as the blood exhibited no size, and there was a suspicion that the disease originated in contagion. The patient had taken antimonials without any apparent effect, and after watching the state of the thermometer, with the bulb at the axilla, upwards of an hour, though the mercury had sunk a single degree in that interval, it stood at the end of the time as high as 106°. In this state of things we resolved on trying the cold affusion, and every thing being prepared, he was stripped naked and lifted out of bed. As we were about to throw the water upon him, it was observed that a sensible perspiration had broken out all over him, but the heat being so great, we persisted in our purpose, and four gallons of fresh water of the temperature

of

of 60°, were dashed upon him; the effects were altogether surprising. On replacing him in bed, the mercury in the thermometer (the bulb at the axilla as before) rose to 98 only, and the burning heat of the extremities was converted into a coolness that was rather alarming; the pulse had sunk in frequency to 90, but was full and steady. Gentle frictions were applied to the legs and feet, but they were not long continued, for the general warmth speedily returned; the heat in the trunk of the body rose in about an hour to 100° and the pulse to 100. His delirium went entirely off; the fur on his tongue speedily disappeared; and twenty hours afterwards he was found free of every complaint but debility.

Subsequent experience has however convinced me, that though in this case the termination was so happy, the cold affusion was not unattended with hazard. Sweating had commenced, and the heat was sinking. It had perhaps sunk more at the moment when the affusion was performed, than was indicated by the thermometer, for the bed clothes often keep the body from cooling under sweating, to the degree that would otherwise be produced. In this case when the surface is suddenly exposed to the external air, the heat sinks rapidly. If the sweat had continued an hour longer before this remedy was used, the heat would have been still

still more diminished; a torpor of the vessels of the surface, and of the extremities, would have been produced, followed by a great, and probably a dangerous re-action of the centre. This observation will be illustrated in the sequel.

In recommending the affusion of cold water as a remedy in fever, an express exception is therefore made against its use during the feverish chill, or after the sweat has begun to flow profusely, and more especially after it has continued to flow profusely for some time. An exception is also made against its being employed in the latter end of fever when the strength is much exhausted, and the heat is sometimes as low or lower than the temperature of health. While, however, the heat rises one or two degrees above the healthy standard, this remedy may be used even in the latter stages of fever. I have employed it with advantage on the 11th, 12th, and 13th days. In instances of this kind it will however be prudent to make the degree of cold very moderate, as has been already observed; and as it is scarcely to be expected, that at an advanced period of the disease the progress of it can be stopped, or its duration much lessened, it may perhaps answer every purpose to employ in such cases the *tepid* affusion. I have indeed often contented myself with spunging the body all over with tepid vinegar, or vinegar and

and water, from the 9th or 10th day forwards; but I have frequently in cases where the heat continued high, directed the general affusion of tepid water, by which the heat may always be speedily and effectually reduced, *when that is the only object in view.*

Under these restrictions, the affusion of cold water may be used with perfect safety in the low contagious fever of this country, and the facts already stated, will show that it is a remedy of great power and efficacy. In the first stages of fever, it appears very generally to cut short the disease almost instantaneously; and even when it fails of this effect, as is usually the case when it is applied in the more advanced stages, it nevertheless moderates the violence of the symptoms, and shortens the duration of the disease.

## CHAP. VIII.

*General remarks on fever. History of a case of fever in which the affusion of cold water was not salutary.*

SINCE the introduction of scientific arrangements into medicine, diseases have been much reduced in number, and their nature has been more clearly understood. This is especially true of continued fever, which is exhibited by Dr. Cullen under three genera only, Synocha, Typhus and Synochus. Of these genera, however, the Synocha, or pure inflammatory fever, without topical inflammation, is confessedly a very rare occurrence in this island; the venerable professor used to declare that he had not met with a single instance of it in forty years' practice.—And the Typhus and Synochus seem to be considered by him as the same disease, modified differently, by the difference of climate, season, and constitution. Both are described as contagious, and as occasionally

ally producing each other. Doubtless the Typhus, or low contagious fever, is the prevailing fever of this island, and of Europe. It is the epidemic of all our great towns, of our jails, hospitals, and manufactories; its origin and progress are clearly ascertained, and its symptoms generally understood. It is to this fever that the preceding observations chiefly apply.

I have my doubts, however, after much reflection and observation, whether we have not simplified too far in our nosological arrangements of fever. The dreadful disease which prevailed lately at Philadelphia, and which now ravages the West Indies (1798), cannot perhaps be included without some violence within our systems of Nosology; and its fatality under all the established modes of treatment, whilst it excites our deepest regret, must serve to abate the pride of modern science. Even in our own island, it appears to me that cases of fever sometimes occur, which cannot be referred with advantage to any of the genera of Dr. Cullen.* The following is a description of a fever of this kind;

* I am aware that all questions respecting nosological arrangement have a tendency to degenerate into verbal disputes, and I willingly avoid them, referring for my accuracy to those who have studied diseases, not in books only, but in the volume of nature.

kind; I have not met with it often, but when I have met with it, it has very generally proved fatal, under the established modes of treatment; and I am sorry to say, that in the only instance of this fever in which I have tried it, the affusion of cold water proved unsuccessful also.

The fever in question does not seem to originate in contagion, or to propagate itself by contagion. I have never been able in a single instance to trace it to that source, nor have I ever found it to be communicated from the patient to any of his attendants. The cases which I have seen have occurred chiefly in the winter season, in persons in the flower or vigour of life, possessed of considerable sensibility of mind, and in the habits of more than ordinary mental exertion. After some days of indistinct catarrhal complaints, the fever comes on (in general after some accidental exposure to cold) with a very violent and long continued attack of chills and rigor, and to this, as is usual, succeeds a state of heat and re-action. The patient complains of intense head-ach and of oppression at the præcordia, with occasional but not severe cough, and with some increase in the frequency of respiration. His pulse is not remarkable as to frequency or strength; his sleep is not particularly disturbed; and for some days the complaint goes on as if produced

duced by catarrhal fever. From the first, however, there appears a great quickness and impatience about the patient: he talks more rapidly than usual; apprehends you quickly, and answers you instantly. He cannot, however, command his attention long, and is fatigued with the effort. His heat, which was at first moderate, becomes very great on the 7th and 8th day, reaching 107° or 108° of Fah$^{t}$.; he becomes delirious and talks incessantly. Throughout the fever, his senses of hearing and taste are uniformly acute, and this is true also of his sense of feeling. Great as his heat is, he is much alive to the impressions of cold on the surface of the body, and shrinks from them. At times he appears surprisingly calm and natural, gets out of bed and dresses himself, insisting that he is well. Often he starts up suddenly in bed and opens his curtains, seeming to look round the room for some person he supposes present; and sometimes he rings the bell violently, if within his reach, without apparent object. Indistinct conceptions rise and vanish in his mind, and the impressions of sense are confounded with the ideas of imagination. As the fever advances, the respiration becomes more hurried and laborious, the pulse more frequent and feeble; and towards the latter end of the disease, but not before, sweats break out, at first partial, and at length general and profuse, which however, though they reduce the heat,

heat, do not otherwise relieve him. The pulse sinks; the body is covered with petechiæ; wine, bark, opium, and blisters, afford no relief: the patient dies on the twelfth or thirteenth day of fever, and after death the body runs rapidly into putrefaction.

I have seen this fever treated by venesection and antimonials in the early stages, with a strict attention to the antiphlogistic regimen; and by bark and cordials, as the strength began to decline; but without success. I have also seen it treated from the first on the same plan as typhus, but with an equally unfortunate issue. In a case of this fever which occurred lately, I made use of the cold affusion, and as the mercury rose in the thermometer with the bulb under the tongue, to 107°, I employed this remedy with some degree of confidence. The effects did not correspond with my former experience or with my hopes. The patient felt the cold most acutely, but was not relieved. His pulse did not diminish in frequency; his heat subsided very little, and that for a few minutes only; neither diaphoresis nor sleep followed. This remedy was not repeated, but the surface of the body was spunged from time to time with vinegar, without however producing sensible benefit or refreshment.

I have

I have already mentioned that the affusion of cold water is not to be used after a profuse perspiration has taken place; and that it is not to be used in the cold stage which begins the paroxysm of fever, nor till the hot stage be fairly formed. In the typhus, however, this last restriction seldom requires us to wait long; the affusion may be used in general in twenty-four hours from the original attack, and often much sooner. The case I have just related is the only instance, out of many hundred trials, in which I found, that even on the sixth day of fever, with the actual heat of the body far above the temperature of health, the affusion of cold water was neither salutary nor refreshing. I have, however, to observe, that notwithstanding the great heat of the body, producing the utmost restlessness and anxiety, the *sensation of heat* was interrupted by chilliness on the slightest application of cold, and that the surface and extremities not only felt chilly, but grew cold, even on the accession of the external air. In reality, through the greater part of the fever, the state of the patient had a considerable resemblance to what we see in the paroxysm of an intermittent, when the cold stage is terminating, but the hot stage not fairly formed—when the heat, as well as the blood, is accumulated in the centre of the system, and the vital power is struggling to give them that propulsion to the superficies, which ter-

minates in profuse perspiration, and carries off the disease. I have little doubt that immersion in the tepid bath of the temperature that feels comfortably warm to the skin, continued for some time, is the proper remedy in the fever I have described, as it doubtless is in the struggle of the paroxysm of intermittent; and when an opportunity offers, I mean to ascertain the truth on this point. But this opinion will be illustrated when we come to speak of the warm bath more particularly.

If any one should contend that the fever I have described is in reality only a variety of the typhus, or synochus, I shall not be disposed to contest the matter. The question concerning *identity*, leads to endless disputes in every branch of science where it occurs, and he must know little of nosology, who supposes *it* has yet received a consistency, that would render such a discussion profitable. It is sufficient for me to observe, that the symptoms of the two diseases are in a considerable degree different, though with that general similarity that belongs to all cases of fever; that the state of the nervous system as to impressibility is widely different; and what is of most importance, that the methods of treatment, which according to my experience, almost invariably succeed in the one disease, are unsuccessful in the

the other. Every practitioner knows, that in typhus, the sense of hearing is generally obtuse; and the same may be said of the taste, smell, and touch; whether the observation is applicable to the sight also, and under what restriction, appears to me doubtful. The acuteness of all these senses in the fever which I have described, is very remarkable, and particularly in regard to the sensibility of the surface. I have observed this symptom to be produced by several narcotics, and by some poisons. It is very remarkable in the hydrophobia; and in the last days of a person who died of inanition, the senses of touch and vision were extremely acute.*

* This case will be found in chap. six. p. 304.

## CHAP. IX.

*Of the use of the affusion of cold and tepid water in small-pox, with cases.*

THE singular degree of success that, on the whole, attended the affusion of cold water in typhus, encouraged a trial of this remedy in some other febrile diseases. Of these the small-pox seemed more particularly to invite its use. The great advantage that is experienced in this disease, by the admission of cool air, seemed to point out the external use of cold water, which being a more powerful application, might be more particularly adapted to the more malignant forms of small-pox. The result corresponded entirely with my expectation. Of a number of cases in which I witnessed the happy effects of the affusion of cold water in small-pox, I shall give the following only.

### CASE I.

In the autumn of 1794, J. J. an American gentleman, in the 24th year of his age, and immediately

mediately on his landing in Liverpool, was inoculated under my care; the prevalence of the small-pox rendering it imprudent to wait till the usual preparations could be gone through, or indeed till he should recover from the fatigues of the voyage. He sickened on the seventh day, and the eruptive fever was very considerable. He had a rapid and feeble pulse, a fœtid breath, with pain in the head, back, and loins. His heat rose in a few hours to 107°, and his pulse beat 119 times in the minute. I encouraged him to drink largely of cold water and lemonade, and threw three gallons of cold brine over him. He was in a high degree refreshed by it. The eruptive fever abated in every respect—an incipient delirium subsided, the pulse became slower, the heat was reduced, and tranquil sleep followed. In the course of twenty-four hours the affusion was repeated three or four different times at his own desire; a general direction having been given him to call for it as often as the symptoms of fever returned. The eruption, though more numerous than is usual from inoculation, was of a favourable kind. There was little or no secondary fever, and he recovered rapidly.

In situations where the eruptive fever of small-pox is clearly distinguishable, and where it does not abate sufficiently on the admission of cold air, the

the affusion of cold water may be resorted to with confidence and safety, regulated however in this application, as in every other, by *the actual state of the patient's heat, and of his sensation of heat.* In the confluent small-pox, however, after the eruption is completely formed, this remedy cannot perhaps be used with advantage. The following case will illustrate this position.

H. A. aged 23, an American mariner, fell under my care (*Dec.* 7,) on the third day of the eruption of the small-pox; that is, on the sixth day of the disease. His pulse 114 and feeble, his heat 109°. His head, back, and loins, ached severely —thirst great—skin livid—small-pox confluent.

He was put on a milk diet—gentle mercurial purgatives were ordered from time to time, and an opiate every night at bed-time. Lemonade was given largely, at first by itself, and afterwards mixed with wine, and the affusion of cold water was directed in the usual way. In ten minutes after the affusion, the pulse was 96, the heat 98°; the livor of the skin was much diminished, but the pains were not relieved.

*Dec.* 8. Noon—Pulse 96, soft and regular—thirst gone—respiration slow and natural—heat 97°.—The affusion was ordered to be repeated; ten minutes after, pulse 84 and feeble—heat 84°.

*Dec.*

*Dec.* 9. Noon—Pulse 88, heat 93°—the cold affusion was not repeated in this very reduced state of heat; the decoction of bark was ordered, and a pint of wine daily in lemonade.

*Dec.* 10. Noon—Pulse 116, and full—heat 98°, respiration still easy—expectoration considerable, and viscid—thirst less—eyes quite closed—head swelled—a complete union of the pustules on the face.—Bark and wine continued, with the opiate at night.

*Dec.* 12. pulse 118—heat 96°. A bucket full of water of the temperature of 92° was poured over him. He appeared refreshed at the moment; ten minutes after, pulse 112, heat 94°. Complained of being chilly. Respiration still easy—free of pains, and his face less swelled. Complained of his throat. A blister was applied to it all round.

*Dec.* 13. Noon—Pulse 118—heat 96°—respiration still free, but his throat very sore. Medicines were continued, but the affusion of tepid water was not repeated.

*Dec.* 14. Noon—Pulse 138—heat 100°—respiration had now become laborious, and the expuition difficult. The throat was much swelled. He

was

was frequently spunged with tepid water, and the medicines continued.

*Dec.* 15. Noon—Unfavourable symptoms increased.

*Dec.* 16. Noon—Vomiting came on, which was relieved by opium. His senses and his intellect remained acute till within an hour of his death, which happened at eight o'clock in the evening of this day.

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If this case be more detailed than seems necessary, let this be excused, as it is the first in which the actual heat in confluent small-pox has been recorded. It is here given accurately from the period when the disease came under my care.

In regard to the effects of the cold affusion, it may be observed, that this remedy was not used during the eruptive fever, nor till three days after the eruption had appeared, and the character of the disease was decided. In the stage in which it was employed, the fever and the heat were abating, as is usual after the eruption; and in all cases in which the heat is sinking, the application of cold must be made with great caution, as has already been mentioned. After the second affusion (on the 8th) the heat sunk below its natural standard,

dard, and continued below it for some time; so that this remedy became inadmissible. The disease went through its usual course. The tepid affusion on the eighth day of the eruption (*Dec.* 12) was used in part to wash off variolous matter, and in part to produce refreshment. The heat, which was before 96°, sunk two degrees, so that it could not with safety be continued, for experience has proved, that the tepid affusion is a powerful means of diminishing heat. The heat rose again with the secondary fever, and the patient died of the affection of the throat, as I believe is general in the confluent small-pox.* It will be at once perceived, on the principles already laid down, that in a disease like this, the affusion of cold water could only be essentially useful during the eruptive fever. It is during the eruptive fever that the quantity of the assimilation is determined, as well as its kind. This is, I believe, invariably found to bear an exact proportion to the eruptive fever, and whether we consider the eruptive fever as the cause or effect of the assimilation, there is every reason to expect, from the laws of the living system, that the diminution of this fever will diminish the quantity, and meliorate the quality of the variolous eruption.

In

* See Zoonomia, vol. ii. page 237.

In the case just related, the heat during the eruptive fever (judging from trials in similar situations) had risen to 106° or 107°;* but it had sunk to 100°, before the cold affusion was employed. It may easily be conceived that this remedy could have been employed to a much greater extent, and that its effects would have been far more salutary, if it had been used throughout the previous fever. That it would have essentially altered the character of the disease, I presume not to assert. This however, I can declare, that in all the cases in which I have used the affusion of cold water during the eruptive fever, however severe the symptoms may have been, these symptoms instantly abated, and the disease assumed a benignant form. The case of Mr. Johnston (Case I.), already given, will illustrate this observation; and six or seven others I might adduce to the same purpose. As yet my experience extends no farther.

The inoculation of infants is so very rarely followed by any serious disease, that as far as respects them, the affusion of cold water may be seldom required. The inoculation of adults is not, however, quite so safe. Inoculation is seldom

* 1803. I now believe that the heat does not rise so high in any stage of confluent small-pox. See the *Additional Reports*.

dom indeed performed in our island on adults that are natives, but foreigners frequently require it; and in Liverpool, our intercourse with America renders it often necessary to perform it on adults from that continent. We may also observe, that when the natural small pox is epidemic, the eruptive fever will be generally distinguishable, and wherever it is distinguished with symptoms of violence, instead of trusting to cool air only, the cold affusion, or cold bath, is strongly recommended. To our brethren across the Atlantic this is more especially addressed.—In America, as well as many parts of the old continent, in consequence of the neglect of early inoculation, the natural small-pox at times spreads alarm and devastation throughout extensive districts. In this island the ravages of the natural small-pox are on the whole very great, yet they occasion little disturbance or alarm. The practice of inoculation among the more opulent classes of society, keeps up the contagion in all our populous districts, and at the same time by relieving these classes from the apprehension of the natural-pox in their own families, prevents them from opposing the casual progress of the disease among the inferior orders, who want the knowledge and the combination necessary to the use of the means of prevention among themselves. Though therefore it is demonstrable, and has indeed

deed been demonstrated, that the destruction of the natural, or rather the casual small-pox, might be entirely avoided, yet, as it falls almost wholly on the families of the poor, and as it has been an evil that has been long, and that is familiarly known, we submit to it through habit, as if it were inevitable.* The alarm produced by the casual

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* The observations on the possible prevention of the casual small-pox, will doubtless suggest to the medical reader the "Sketch of a Plan" for that purpose, published in 1793, by my respectable neighbour, Dr. Haygarth. That this plan is in itself practicable, and that it would be effectual, I have little doubt. Unfortunately, it requires the assistance of government (as I remarked at the time) and this I fear is a powerful objection. It is possible however that some scheme of this kind (including I hope all contagious diseases) may one day or another be attached to some comprehensive plan for the management of the poor.

The theory that suggested Dr. Haygarth's plan, but which formed no essential part of it, involved him in a discussion on the length of time that variolous matter may be exposed to the atmosphere, and retain its infectious quality. On this occasion, as there was some difference of opinion between us, he proposed several experiments to me, which would doubtless have decided the question. *(See p. 459, 460, of "A Sketch, &c.")* It was fully my intention to have undertaken some experiments such as he mentioned, and I even commenced them, but as my attention was forcibly drawn to other subjects, these experiments, which required extreme accuracy, were not completed, and my engagements have never since permitted

casual contagion is therefore seldom so great in our large towns, as to give a practitioner frequent opportunities of treating the eruptive fever of the confluent small-pox, the only stage of that disease in

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mitted me to recommence them. I have, however, since that time, inoculated with matter at different periods from its being taken from the patient, and the result is as follows :— The length of time which variolous matter exposed to the air retains its contagious quality, depends on its superficies. If it be spread very thin on a piece of flat or convex glass, it loses this quality much sooner than when it is collected in a mass. Spread thin upon glass, it sometimes disappoints the inoculator at the end of twenty days, though not generally; and I have known it succeed in communicating the disease, even when diffused over a large surface, at the end of seventy-three days. *(See Dr. Haygarth's Sketch, p. 447).* But I find from the experience of others, as well as from my own, that this is not common. The instance in which I found variolous matter to retain its infectious quality longest, was the following: On the second of February, 1792, I took a considerable quantity of this matter on a piece of window glass, keeping it as much together as its fluidity would admit. It was exposed immediately to a stream of air, and the surface was speedily dry. On the second of March following, after moistening a portion of it with a little water, I inoculated three patients, and all with success. In the course of the summer I inoculated with another portion of it, previously liquified by the addition of warm water, and with success as before. On the twentieth of July, 1793, I again used a portion of the same matter in the same way, and again with success; but this success was long doubtful, and it was not till the twenty-second day after the operation, that the patient sickened.—I used the same

in which medical treatment is likely to be of much avail. Where such an opportunity does occur to the judicious reader of these pages, it is hoped that the affusion of cold water, or the cold bath, will not be neglected. The Chinese, it is said, have long followed this practice with extraordinary success.*

The

same matter in June, 1794, when it entirely failed me. It is now by me, and is not mouldy, nor any ways changed in its appearance. Variolous matter kept some time is certainly slower in producing the disease, even where it does succeed in the end.

1806. The happy discovery of the certain method of preventing, and of finally annihilating the small-pox, given to the world by Dr. Jenner, deprives experiments of this kind of much of their utility and interest.

* Sir William Watson, in his tract, entitled, "Account of Experiments on the most successful methods of inoculating the small-pox," published in 1761, mentions the case of a young woman, who, in the absence of her nurse, got out of bed delirious during the eruptive fever of small-pox, and threw herself into the new river near Islington. She was discovered floating on her face; and when taken out of the water, had not the least appearance of life. She was recovered, however, by the usual methods, and afterwards passed well through the disease.

In some parts of Bengal, according to Mr. Ives, inoculation is practised among the natives.—After the operation is performed, the patient is ordered to bathe in cold water thrice a day,

The use of the cold affusion in fever was common among the physicians of the infirmary, (Dr. Brandreth, Dr. Gerard, and myself), while fevers were received into that hospital, and its extraordinary success there has rendered it in Liverpool familiar in private practice, and extended it, as I have already mentioned, to several other febrile disorders. The scarlatina anginosa has for many years prevailed in Liverpool, and though in general mild, has, as at particular seasons, been malignant and fatal. To this disease the affusion of cold water has lately been applied by my friend Dr. Gerard, and the particulars that follow are mentioned on his authority.

In the latter end of December 1796, all the children of a family in his neighbourhood, five, in number, had been attacked in succession with scarlet fever; four of these were recovering, but one was dangerously ill, when the father of the family, with whom one of the children had slept, was himself seized with all the symptoms of the disease.

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a day, and to live on the most cooling diet. When the fever comes on, the bathing is left off, but it is resumed on the second day after the eruption appears, and continued for the three successive days. It is supposed to fill the pustules.

*Ives's Voyage to India, in the years* 1755, 6, 7, *ch.* iv. *p.* 54.

disease. He had excessive pain in his head and back, and flying pains all over him. He had frequent rigors, loss of appetite, and sickness, with some flushing of the face, but without any efflorescence on the skin, or affection of the throat. This was his situation when Dr. Gerard was called in, about sixteen hours after the first attack. An emetic, and afterwards a cathartic were ordered, but their operation was slow and imperfect, and on visiting him, ten or twelve hours afterwards, he was not materially relieved.

Entertaining no sort of doubt of the nature of the attack, and these symptoms foreboding that the epidemic would, in this instance, be severe, Dr. Gerard determined to try the affusion of cold water, from which in typhus he had seen such happy effects. Accordingly the operation was performed, and with a result that far exceeded his hopes. As the patient was much debilitated, half a pint of hot wine was given him after it, and on being put to bed, the symptoms of fever were found nearly gone; a genial warmth diffused itself over the extremities of his body, followed by sensible perspiration and sleep. Next day he complained of a slight degree of head-ach and lassitude; Dr. Gerard therefore ordered the affusion to be repeated, as well as the warm wine after

after it; the symptoms of the disease vanished, and never re-appeared.

A day or two afterwards, a maid who had been hired as an assistant to attend the sick children, and who had been about a week in the house, was attacked by the precise symptoms already related, and which had uniformly ushered in the epidemic. She took an emetic on the first attack with little benefit, and soon after the cold water was poured over her, the wine being administered after it. In this case the remedies were used earlier than in the former one;—they were used once only: the febrile paroxysm was dissolved, and never returned.

The result of these cases communicated by Dr. Gerard, leads to a variety of important reflections. That the affusion of cold water extinguishes the incipient scarlatina as well as the typhus, can scarcely be doubted, and thus this powerful and simple remedy is extended to another, and a most important class of diseases. That the disease may be extinguished without the specific efflorescence of the skin, or affection of the throat is a circumstance not a little curious. It seems to demonstrate that this efflorescent matter is the product of the eruptive fever, and that the fever itself being destroyed in the first instance, the efflorescent

matter is never produced. Thus we are freed from the apprehensions which a false theory might suggest against extinguishing a process by which nature was extricating itself from an acrimony which the system had imbibed.—Thus also our conclusion is supported, that the eruptive fever of small-pox is the cause, and not, as some have supposed, the consequence of the progress of assimilation, and that the diminution of this fever by cool air, and still more by the affusion of cold water, actually diminishes the quantity of matter assimilated, and in certain cases might perhaps wholly prevent the assimilation.* This last conclusion is indeed doubtful, as all analogical inductions must be between different diseases; and particularly where the difference is of this important kind, that the constitution in one case is susceptible of the disease once only, while in the other it may probably receive it indefinitely.† We may also

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* Dr. Cullen has assumed, that in all the various degrees of small-pox, the quantity of matter assimilated bears an exact proportion to the bulk of the body, and that the difference in the nature and quantity of the eruption depends wholly on the permeability of the skin. This doctrine, which in his lectures he extended to all the other exanthemata, is in my judgment, one of the weakest parts of his most valuable work.

† (1803) I was of opinion that the same person might be again and again affected by scarlatina, but experience leads me

also observe, that the prevention of the assimilation of small-pox, by wholly extinguishing the eruptive fever, if it were in our power, would not be advisable, since it must leave the patient exposed to the future influence of that contagion.

I have not had an opportunity of repeating Dr. Gerard's practice in the incipient stage of scarlatina, but after the efflorescence on the surface decides the nature of the attack, I have for the last fifteen months uniformly prescribed immersion in the tepid bath (from 92° to 96°), and with striking benefit. Whether the affusion of cold water is applicable to the other exanthemata, must be left to future experience.

In the cases that I have related to illustrate the effects of the cold affusion, the temperature of the water may be judged of from the season of the year. In general it was from 40° to 50° of Fah^t^. In the epidemic which prevailed in the 30th regiment, the water of the river was employed, as has already been mentioned, which as the season was uncommonly cold, did not,



me to a different conclusion. I now believe that scarlatina, like small-pox and measles, affects the same person once only. In the *Additional Reports*, the reader will see an ample detail of my farther experience of the nature and treatment of this disease.

though in the month of June, exceed 58° or 60°. I have, however, very often used the river water in private practice during the summer months of the last four years, when in general it has been from 65° to 70°, and the effects corresponded with those already described. The solution of fever depends chiefly on the sudden, general, and powerful impression on the sensations, and this impression is less affected by the difference in the temperature from 40° to 65°, as far as my observation extends, than might on a first consideration be imagined. Within these limits the efficacy of this remedy, as well perhaps as its safety, depends on the suddenness and momentariness of its application. The powerful impression on the sensations is much weakened when the water is poured slowly on the body, and as the respiration is suspended or convulsed during this application, as well as during the act of immersion in the cold bath, it might in some cases incur hazard to protract it.

CHAP.

## CHAP. X.

*Of the affusion of tepid water on the surface of the body, in feverish disorders, and of spunging the body with water or vinegar. The affusion of tepid water practised by the ancients.*

I APPLY the term *tepid* to water heated to that degree which is warm but not hot to the sensations, and which in the way of affusion is from 87° to 97° of the scale of Fah'. According to my experience, this term, when the body is immersed, may be applied to water some degrees colder; the reason of which will be easily understood by those who reflect, that under immersion no evaporation from the surface of the body takes place. At first I imagined that the tepid affusion might be beneficial in cases where the heat of the body is below the degree necessary to render the cold affusion safe. I employed it therefore in those stages of fever where the heat did not exceed the temperature of health. A little experience however convinced me that this practice required strict attention, for I found, that in many cases, at least, the heat of the living body is

is lowered as speedily by the affusion of tepid water, as by the affusion of water that is cold:—if I mistake not, in some cases the heat is lowered more speedily by the tepid water. To those who reason respecting the heating and cooling of the living body in the same manner as respecting inanimate matter, this observation will appear paradoxical; I assert it however from actual observation, and a little reflection will explain the phenomenon. The evaporation from the surface is more copious from the tepid affusion, and on this the cooling of the body very much depends. But this is not all; the tepid affusion is little if at all stimulating, and does not, like the cold affusion, rouse the system to those actions by which heat is evolved, and the effects of external cold are resisted. Where the object is to diminish heat, that may be obtained with great certainty by the repeated use of the tepid affusion, suffering the surface of the body to be exposed in the interval to the external air—and if the beams of the sun are excluded, and a stream of wind blows over it, the heat may thus be reduced where cold water cannot be procured; even in the warmest regions of the earth—on the plains of Bengal, or the sands of Arabia. I have accordingly employed the tepid affusion very generally in those feverish affections where the morbid actions are weakly associated, depending rather on the stimulus

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of preternatural heat, than on contagion, miasmata, the morbid contents of the stomach and bowels, or local inflammatory affection. Of this kind are a great part of the feverish affections of children, in which the tepid affusion is a valuable remedy. It very generally produces a considerable diminution of heat, a diminished frequency of the pulse and respiration, and a tendency to repose and sleep. I have used it also in feverish disorders of various kinds where the lungs are oppressed, and the respiration laborious, and where of course the oppression might be dangerously augmented by the sudden stimulus of the cold affusion. It is also applicable to every case of fever in which the cold affusion is recommended, and those may receive much benefit from it, whose fears or whose feebleness deter them from that energetic remedy. I have not however found its effects so permanent as those of the cold affusion, and I have never seen it followed by the total cessation of regular fever, as often occurs after the cold affusion. In the hectic fever, however, where the actions are less strongly associated than in synochus or typhus, the paroxysm is sometimes completely extinguished by the affusion of tepid water* on the commencement

* See this fact mentioned in the Zoonomia, vol. ii. p. 296, where a relation is inserted by Dr. Darwin, of the author's (Dr. C's.) own case of hereditary consumption.

mencement of the hot stage. In the hectic paroxysm, the heat seldom rises more than two degrees above the temperature of health in the trunk of the body, and three or four degrees on the extremities. By moistening the palms of the hands and the soles of the feet with vinegar, its effects may be moderated, for it is from the sensation of heat in the extremities, that the stimulus to the system is chiefly derived; and this practice ought not to be neglected, if the tepid affusion is not employed generally. In all cases of fever indeed where the burning heat of the palms of the hands and soles of the feet is present, this method of cooling them should be resorted to; it is uniformly safe and refreshing. I have not employed the cold affusion in the hectic paroxysm. This disease generally adheres to a debile system; the body parts with its heat in it easily; and the lungs being always affected in the pulmonary hectic, the sudden application of cold to the surface might produce unpleasant, and perhaps dangerous effects on the respiration. Neither have I tried it in peripneumony, or measles.*

When the affusion of water, cold or tepid, is not

* (1803) The reader will find some account of its being accidentally used in a few cases of measles, in the *Additional Reports*.

not employed in fever, benefit may be derived, as has already been mentioned, though in an inferior degree, by spunging or wetting the body with cold or warm vinegar or water. This application is however to be regulated like the others, by the actual state of the patient's heat, and of his sensations. According to my experience, it is not only less effectual, but in many cases less safe; for the system will often bear a sudden, a general, and a stimulating application of cold, when it shrinks from its slow and successive application.*

I have also used the affusion of *cool* water as a remedy

* I have purposely avoided entering on the general operation of cold, in hopes of being better prepared for the subject at some future opportunity. In the mean time I have continued the use of such terms as are most intelligible, and most consonant to the true doctrines as far as I perceive them. To speak of cold in any form acting as a stimulus, seems however to some learned friends, not merely an error, but an absolute contradiction in terms. Heat, they affirm to be the universal stimulus, and cold being merely the privation of heat, *must* in their opinion, always have a sedative operation. As well, it is said, may darkness, which is the privation of light, stimulate the eye, as cold, which is the privation of heat, stimulate the general system. This observation, which has more importance with me from the quarter whence it comes, than from any intrinsic weight it possesses, leads me into one or two general remarks.

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affusion. Like the cold affusion its application should be sudden and momentary, when the object is to increase the tone of the system, or to dissolve

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The stimulating action of cold, though short in duration, is powerful in degree. In the torpor of convulsion, when weaker stimuli are unperceived, the affusion of cold water on the naked body will often excite the dormant sensibility, and introduce a new action throughout the nervous system. In the apoplectic state brought on by the fumes of charcoal, this remedy is of all others most efficacious; when dogs are suffocated in the vapour of the *Grotto del Cani*, it is well known that they are recovered by plunging them in the adjoining lake. And in other animals, when the last motions of life are apparently over, the same application will sometimes, as I have observed, renew the contraction of those fibres that seemed before fixed in death.—This observation I hope to illustrate at some future opportunity.

Can darkness stimulate the eye? No—Darkness produces no sensation. Can cold be rendered stimulating? Surely—After what has been observed no one will deny it: unless indeed it should be said, that it is not the cold that stimulates, but the sensation which the cold produces; a point that it would be a waste of time to dispute. It is the stimulant power of cold that renders it so difficult to employ it in inflammatory diseases. (See note at the end of chap. xv.)

(1803) The above note was written in answer to some observations in a letter from Dr. Darwin.—He expressed an intention of noticing it in the next edition of his Zoonomia, but nothing of the kind is to be found there.

dissolve a morbid catenation; where it is employed to moderate inordinate heat, it may be used more slowly, provided it does not interrupt the catenation on which respiration depends.*

The practice of giving cold water as a drink in fevers, was common among the ancients, and immersion in cold water they occasionally employed; but the affusion of it on the surface of the body seems to have been wholly unknown. Ablution of the surface with cold water in feverish diseases, has been traced under different modifications among the practices of the rude nations of Asia and of Africa by modern travellers, particularly by Sir John Chardin, and Mr. Bruce; but it was first brought into notice in Europe during an epidemic fever which prevailed at Breslaw, in Silesia, in 1737, as appears from the dissertation of J. G. de Hahn, under the title of *Epidemia verna quæ Wratislaviam, anno* 1737, *afflixit*; to be found in the Appendix to the *Acta Germanica*,

* I borrow this term (catenation) from Dr. Darwin.—It is employed to express a number of actions linked together, nearly in the sense of *association*: the *sympathies*, as they are usually called, are included under this term, and many *associated* actions to which the word sympathy has not been applied, as the connexion between the heart and the lungs, the stomach and the heart, &c. (See *Zoonomia*, vol. i. sect. xvii.)

*manica*, vol. x.* While the laws by which the affusion of cold water ought to be regulated were not

* De Hahn says that he is the better able to describe this disease, as he himself was seized with it; and was cured by "peregrina illa multis visa medendi methodus." He begins with an account of the causes of the epidemic. That first mentioned is the state of the weather. In the month of May, 1736, after a very promising spring, the weather became wet and cold, and the fall of rain was so considerable during June, July, and part of August, as to lay a great part of Silesia under water. The wind blew chiefly from the North-West.—The harvest was almost entirely destroyed. In consequence a famine prevailed throughout the province, of which the author gives a dreadful account, and this he mentions as the second cause. The third cause was the vitiated air. The unburied carcases of persons slain in battle were suffered to putrefy in the atmosphere, and the stagnation of the waters in the low grounds filled it with marsh effluvia. Even the cattle suffered severely from this state of the air, and from the want of pure water to drink. The epidemic began in the ensuing spring. He gives a number of cases. The first is that of a farmer in the neighbourhood of the city, of forty years of age. "Comites febris erant subitus virium lapsus, capitis & præcordiorum dolor gravis, fluxio alvi, pervigilium, inquies, delirium. Quæ insuperabilia morbi symptomata excipiebat die undecimâ his ipsis insuperabilior obitus." The second case he mentions occurred in the city. It is that of a delicate woman, of thirty years of age, previously afflicted by severe misfortunes. The symptoms were as follows. "Subito elanguida febriebat. Dolor capitis illico atrox. Sitis, & secundo die, importuni menses. Exin vomitus bilis, alvusque biliosa. Sputa viscida. Syncopticus rigor. Ardor partium

not understood, the use of the remedy must have been extremely hazardous, and the fatal consequences

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partium internus. Linguæ quasi candenti ferro compressæ, sicca glabrities & restrictio. Vox arentibus faucibus nulla. Angina sine tumore pessima. Repetebant interdum tenacis pituitæ vomitus alvique fluxio. Os sine medelâ siccum. Iactatio. Supinus torpor. Mors inter convulsiones gemibunda." The author proceeds to detail four other cases similar to the above, which also terminated fatally. Two females who caught the infection, escaped in consequence of critical eruptions.

At length the alarm became general. The attention of the magistrates was roused, and from a strict inquiry it was found, that more than double the usual number of deaths had occurred at Breslaw in that year. All the usual methods of practice were of no avail. Whether bleeding was employed or abstained from, the disease was equally mortal. In this distress Dr. De Hahn determined to try the effects of the external use of cold water. The first case in which it was employed, occurred in the month of April; it is related as follows. "Mercator xxxii annos natus, floridus naturâ, mox febriens, capitisque dolore & nauseâ affectus. Permittentibus id viribus, alterâ die mittebamus sanguinem, qui inflammatorius. Vespere vomebat aeger, tertiâ die maculosus. Maculæ morillis aemulae, inconstantes. Mador. Mox præcordiorum intolerabilis dolor & delirium. Quasi ex temulentiâ vacillans extra lectum vagabatur aeger. Oculo altero dilatato, altero connivente, utroque gramioso & caliginoso; linguâ lævi, aridâ, candente, sputo resinoso; urinâ biliosâ."—The usual remedies failing, on the eighth day they had recourse to ablution. "Tum ad externas illas humectationes confugiebamus, indefessâ operâ spongiis

quences of its improper application, we can easily believe to have prevented its gaining any ground on

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spongiis omnem corporis ambitum demulcentes. Id consequebamur, ut sensim sputa resinosa fierent magis mobilia; ut flaccida et fusca facies conspiceretur magis turgidula; ut *felicius procederet blandus mador;* ut resipiscereret, hactenus vel loquax nimium, vel taciturnus ex delirio æger." The author then relates a case of a similar nature, in which, the ablutions being rejected, the patient died. Next follows the case of a lady, a relation of his own, forty-three years of age, whose symptoms appeared of the most dangerous nature. In this case the attack was sudden. "Inter domesticas occupationes quasi conquassata concidebat. Facies primo momento Hippocratica; artubus glacie perfusis & tremulis. Horror repetitus ad quemvis aeris attactum. Vomitus post quævis ingesta. Abdominis turgor. Videbatur mihi advolanti non morbo corripi aegra, sed siderata mori, vixque amplius cum morte colluctari. Pharmaco, ob vomitus, non erat locus; neque venæ sectioni, ob algida & emarcida membra. Die 2dâ, jugi abluitione artuum glacies diffluebat inter gemibundos angores. Sitis tandem invitabit potum. Calorem excipiebat æstus. Clysma solvebat partes abdominis internas. Quartâ die, magis sibi conscia aegra caput quasi a percussione dolere conquerebatur. * * * Octavâ demum die, certa redituræ sanitatis spes, duorum tandem mensium confirmata."

The author next relates the case of a man of sixty-four, whom he attended, with two other physicians, and on whom the affusion of cold water, or ablution of any kind, does not seem to have been tried. This person died on the 10th day, and our author himself was immediately after seized with the disease, having probably caught it from this patient. He was in

on the continent, or its having been adopted in Britain. These laws are now, I hope,

 ascertained

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in his 44th year. " Die 1mo, Persenseram inter vix superabiles labores, infestum aliquamdiu nuchæ dolorem. Hic isto mane solito ferventior, febrem epidemicam invitabat absque prævio horroris sensu. Sævus ille dolor ex nuchâ ortum ducens—totum tunc ambiebat caput. * * * Fervebat caput, frigentibus pedibus, spasmo ad abdomen rigidis. Augescente de momento in momentum dolore, horror aderat ad quemvis aeris attactum. Languor mox ab initio extremus. Nox inquieta & sudans. 2do, Oculi gravissime dolentes * * * 3tio, Parca mane remissio dolorum, mox intensiorum; febre, prævio tempore, auctâ. Nox clamoso nuchae & capitis cruciatu atrox. 4to, Post hanc dies nocte pejor. Pedum glacies nondum egelata." These symptoms increased daily. On the seventh day there was great subsultus tendinum, and the whole body was covered with petechiæ. In these circumstances recourse was had to ablution with cold water. " Ab hac die abluitio frigida universalis in usum vocata cum antea particularis tantum obtinisset. 8vo, Pulsus tremulus observatur. Gemitus a dolorum ferociâ perennis. 9mo, Vomitus grumum cruoris emoliebatur. 10mo, Obtinebat tunc rerum ab aliquo mentis deliro stupore incuria. 11mo, Sudor, armistitium aliquot horarum producens, opportuno corticis usui locum concedebat, cum jam exitialis languor quævis pessima minaretur. Loquela difficilis & balbutiens. Angina apthosa. Stridor dentium. 12mo, Risus sardonicus. Spasmi cynici. * * * Id interim consequebamur corticis usu, ut quotidiana exacerbatio tardius, & die decimâ quartâ, quæ tota comatosa fuerat, sub noctem demum ingruerit. Sed gravitate tarditatem compensabat, violenti frigore totum corpus quatiente. Mox algidus aderat sudor, deficiente loquelâ;

ascertained by ample experience, and practitioners will, I trust, find themselves directed in safety

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loquelâ; demissa citra voluntatis imperium urina." From this apparently desperate state, De Hahn, however, recovered, and chiefly, as he apprehends, by the ablutions of cold water, which were employed even in this advanced stage of the fever. Almost from the beginning of the fever he indulged freely, according to the plan he had followed with others, in liquids, consisting chiefly of water, rendered grateful and cooling by the addition of lemon juice, nitre, &c. and every night he took a moderate dose of opium, from which he found the greatest benefit. From all heating alexipharmics he entirely abstained, having the greatest repugnance to them. On the seventh day, the general ablutions with cold water were begun. The method employed seems to have been that of sponging the surface, and this appears to have been continued for some time together. The effects are described as follows. "Sancteque testor, nunquam non refici ad breve temporis momentum languentes marcidae cutis fibras me persensisse. Succedebat, repurgatis hac ope poris, perspiratio liberior, ac sudor modestus; videbanturque cutis obturacula promptius spongiâ remota, quam pharmaci interni virtute. Recreabat præterea hiantia & purum aerem sitientia cutis ora, liberior aeris accessus." This account of the effects of the practice, corresponds with our experience at present. Dr. De Hahn found the advantage of a plentiful supply of fresh air, at that time little understood throughout Germany or Europe, and he secured it by keeping his windows open. He changed his bed-clothes and linen frequently; and he occupied one bed during the day, and another during the night, a practice which moderated the heat, and often produced great

safety in the future use of this powerful remedy.



great refreshment. This practice deserves, indeed, to be followed whenever it can be done with convenience. Experience convinces me that it always produces much comfort under feverish heat and irritation, and that it often procures tranquil sleep. Where a change of bed cannot be obtained, the method of Dr. Franklin, of exposing the body to fresh air, and ventilating the bed-clothes, ought to be resorted to. De Hahn took the bark on the 11th day, and found it very grateful to his stomach, and agreeable to his sensations. That day he took five drachms of the powder. On the 12th, he took four drachms; on the 13th, three only; and on the 14th, only one; his relish for it having gone off. The bark seemed to moderate his symptoms, which returned with violence on leaving it off, and on the 15th day he found himself on the point of death. At this very time, however, he persisted in the cold ablutions, and to them he ascribes his recovery. "Juges illae abluitiones, quibus hucusque recreatus fueram, eâ ipsa die, quâ mori videbar, non negligebantur; licet enim totus algidus algido sudore perfunderer, non secus ac liquefactae glaciei immersus, frigidâ tamen abluebar; marito serviturâ charissimâ conjuge lethales madores ocius deluente." From this time the symptoms became less violent, but his recovery was slow and difficult. On the 18th day he was still delirious, and syncope came on when he attempted to get out of bed. His appetite, however, began to return, he had copious sweats, and fell into profound sleep. On awakening he felt a great aversion to noise, and every thing appeared new and extraordinary. On the 36th day cholera came on, but was subdued. On the 48th there was a desquamation of the skin, and a falling off of the nails. He concludes this account of his own case in the following words, "Morbi tandem reliquiæ menstrua equitatione feliciter subactae."

## The affusion of tepid water in febrile diseases was not unknown to the ancients, though seldom employed

De Hahn was attended by his father, also a physician, under whose direction the ablutions were performed, and by whom the greater part of the symptoms must have been recorded. In consequence of this success, cold ablutions were employed with others, and many were saved by them in circumstances apparently the most desperate.

It is evident, from this abstract, that De Hahn was not regulated in his use of external ablution with cold water, by rules similar to those which I have ventured to lay down from several years' experience. Instead of pouring the water over the naked body, he applied sponges soaked in cold water to every part of the surface in succession, and seems to have continued the application for some time together; in my judgment the least efficacious, as well as the most hazardous manner of using the remedy. He does not seem, in general, to have used the ablutions till the eighth or ninth day of fever, and till the cases were growing desperate from the failure of other means. At this advanced stage, the ablutions, as might be expected, seem to have been of very inferior efficacy. Yet in the single case, in which, from the impossibility of the patient's swallowing medicines, ablutions were used on the second day of the fever, the recovery was speedy; it appeared certain on the eighth day; and this might have encouraged an earlier trial of the same practice in other instances. But what appears to me most surprising is, that he does not seem to have been regulated in the use of this remedy, either by the actual heat of the patient, or his sensations of heat. In his own case he expressly declares, that the cold ablutions were used on the 15th day of the fever, when he was shivering with cold, and covered with cold sweat; circumstances under which I should

pronounce

employed by them.* It is, I apprehend, new in modern practice, and will be found an important addition

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pronounce it to be in the most extreme degree dangerous. Whether my restrictions as to the use of the cold affusion, and the application of cold in general to the body, be too severe, future observations must decide; but from a general review of the incautious practice of De Hahn, I am not surprised that his boasted remedy, is, so far as I can learn, no longer in use, either in Silesia or in any part of Germany. Besides ablution, clysters of water were found useful in this epidemic, and water was used copiously as a drink. Eruptions of a supposed critical nature frequently appeared. This fever carried off three thousand persons in Breslaw and its vicinity. The means of making this abstract have been furnished me by a friend at Edinburgh, who copied for me the greater part of the memoir of Dr. De Hahn, from the 10th vol. of the *Acta Germanica*, in the College Library; in my first edition called *Acta Not. Cur.* after Dr. Cullen.

(1803.) In my last edition this note was printed very inaccurately, owing to an accidental circumstance—the quotations from De Hahn are now I hope given with sufficient correctness, though I have not had an opportunity of inspecting the printed work. The abstract of this dissertation in Sauvages is imperfect and incorrect, not being taken from the original work of De Hahn, but from the Review of it in the *Journal de Medicine* for 1757.

See *Nosologia Method: Tom.* i. *p.* 334, *4to. edition.*

* Some traces of its use in fever are to be found in Celsus. "Quidam ex antiquioribus medicis, Cleophantus, in hoc ge-

"nere

addition to the list of our remedies. The effects differ considerably from immersion in the tepid bath, as will be more fully explained in speaking of that remedy. Though the affusion of tepid water was little in use as a remedy, it was familiarly practised as a part of their daily regimen, by the Greeks and Asiatics, in the earlier periods of their history. That the Greeks in later times, and after them the Romans, immersed themselves in the baths to which they were so much devoted, is certain; but in the heroic age of Greece, the affusion of tepid water seems to have been the mode in which this luxury was enjoyed. Abundant proofs of this are to be found in Homer, particularly in the Odyssey, that admirable record of ancient manners. Venus, after her public disgrace, is described as flying to the groves of Paphos, where she is *laved* by the Graces, and the improvement

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" nere morborum, (tertian intermittents) multo ante accessio-
" nem caput ægri multâ calidâ aquâ perfundebat, deinde vi-
" num dabat. Quod quamvis pleraque ejus viri præcepta
" secutus est Asclepiades, recte tamen præteriit, est enim
" anceps." *Celsus, lib.* iii. *cap.* xiv. This use of the tepid or warm affusion is altogether different from what I propose, and was not only, as Celsus supposes, a doubtful, but probably an injurious practice. For by chilling the body, it would dispose it to the accession of the paroxysm. The use of the tepid affusion as now proposed, may fairly be considered as a new practice.

improvement derived to her beauty is recorded in a strain of poetry worthy of the subject. *(Odys. lib.* viii. *l.* 362, *to* 367.) Ulysses is represented as *laved* by attendant nymphs in the court of Alcinous, and in the island of Circe the whole process of heating the water, and pouring it over the naked hero, is particularly described.* On this occasion also, nymphs administered to him, who after the ablution anointed him all over with oil; a service well calculated in every respect to increase the pleasure, and to diminish the risque of the tepid affusion.

The Romans under their Emperors, carried the system of bathing to a height of luxury and expense which it never reached in Greece or Asia, as the ruins of their magnificent *Balnea* prove to this day; and the affusion of warm water was one of the methods by which they diversified this favourite gratification.†

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* *Odys. lib.* x. *l.* 358, &c. compare this with the bathing of Telemachus in *lib.* xvii. *l.* 85, &c.

† See *Hieronymi Mercurialis de Arte Gymnast:* lib. i. p. 44 and 45.

## CHAP. XI.

### *Of the internal use of cold water in fever.*

BEFORE I conclude the account of my experience of the use of cold water in fever, it will be necessary for me to say something of its effects when swallowed, on the stomach, and through it on the system at large. Among the ancients the internal use of cold water in ardent fevers is recommended by Hippocrates, Galen, Celsus, and most of the celebrated physicians whose works have come down to us: among the moderns, that extraordinary man, Cardanus, wrote a dissertation in its favour, and to pass over a multitude of inferior names, Hoffman, though with some restrictions, recommends it, not in fever only, but in various other diseases.* In our own country it was proposed as an almost universal remedy by Smith, and a treatise has been written on

* *Hoffmanni Opera*, vol. i. p. 479.

on it under the title of *Febrifugum Magnum*, by Dr. Hancock. In Spain and in Italy the use of cold water in fevers, obtained in the beginning of this century a greater and more general reputation than in any of the other countries in Europe, and at one time, seems to have superseded all other diet, as well as medicine. This treatment was celebrated under the title of *Diaeta Aquea*, and an account of it may be found in the 36th volume of the Philosophical Transactions, by Dr. Cyrillus, a professor at Naples, to which the reader is referred. Besides the internal use of cold water, he mentions the advantage of laying powdered ice or snow on the bodies of the sick.

Nevertheless, the propriety of giving cold water in fevers has been disputed by men of high character, and particularly by the celebrated Boerhaave. His doctrine, that a lentor in the blood is the cause of fever, led him to insist on the use of warm drink, and the danger of cold; and his commentator Van Swieten, though he allows cold drink in some instances, yet in general argues against it.* These learned theorists prevailed in their day over the voice of nature, and the precepts of Hippocrates and Hoffman.—

In

* See *Boerhaavii Aphorism.* Sect. 743. with the commentary of *Van Swieten.*

In the writings of Pringle, Cleghorn, and Lind, we find little or nothing on the subject, though they wrote expressly on fevers; Dr. Cullen mentions cold drink, but gives no opinion on the propriety of its being used, and certainly did not recommend it in practice. He was even doubtful of the extent to which cold air might be admitted.* On the whole, it may be asserted, that the use of cold drink in fever is contrary to modern practice, and that where it is occasionally given, it is administered with caution, and rather permitted than enjoined.

It is not, however, to the doctrine and precepts of Boerhaave alone, that the disuse of cold drink in fevers is to be imputed. The propriety of giving it freely has been at all times controverted, not on the ground of theory only, but from experience of the dangerous, and sometimes suddenly fatal effects of large draughts of cold liquids, various instances of which have been recorded from the earliest periods of medical history. While therefore some physicians have prohibited the use of cold drink in fevers altogether, those who have recommended it from experience of its salutary effects, have introduced various cautions as

* See his First Lines—cure of fever.

as to its exhibition, founded on certain theories, generally fallacious, on the manner in which its deleterious influence is produced. To detail the various opinions that have prevailed on this curious and important point, would be to add another chapter to the ample records of human errors. That the danger arising from cold drink depended on the great difference between the temperature of the liquid and of the body, and that it is therefore to be prohibited when the heat of the body is very great, is an opinion very generally received by the moderns; and among the ancients, though their doctrines were less erroneous, yet while the means of ascertaining the real heat of the living body, and the changes it undergoes, were unknown, it cannot be expected that they should have arrived at the truth on this important subject.

The effects of cold water as a drink in fevers, I was naturally led to examine by my experience of its effects as an external application. I have made this examination with the thermometer in my hand, and with all the attention in my power; and the following results, which will save the reader the fatigue of reading the particulars of various cases and experiments, seem to me to contain all the information necessary to direct our practice.

1. Cold

1. Cold water is not to be used as a drink in the cold stage of the paroxysm of fever, however urgent the thirst. Taken at such times, it increases the chilliness and torpor of the surface and extremities, and produces a sense of coldness in the stomach, augments the oppression on the præcordia, and renders the pulse more frequent and more feeble. Its effects in all these respects are similar to the affusion of cold water on the skin in the same stage of the paroxysm, as described in pages 18 and 38, though inferior in degree. If the thirst is gratified in the cold stage of the paroxysm, it ought to be with warm liquids.

2. When the hot stage is fairly formed, and the surface is dry and burning, cold water may be drunk with the utmost freedom. Frequent draughts of cold liquids at this period, are highly grateful; they generally diminish the heat of the surface several degrees, and they lessen the frequency of the pulse. When they are attended with these salutary effects, sensible perspiration and sleep commonly follow.—These effects are similar to those produced by the affusion of cold water on the surface, as already described, but inferior in degree also. Though various cases are on record of the paroxysm of fever being dissolved by cold water, drunk in this stage of the disease, my experience does not furnish me with any instance of this

this kind.* Indeed, since I became acquainted with the extraordinary efficacy of the affusion of cold water on the surface, I have not trusted the solution of the paroxysm to its internal use. I have however employed cold drink when necessary as an auxiliary. Throughout the hot stage of the paroxysm, cold water may be safely drunk, and *more freely in proportion as the heat is farther advanced above the natural standard.* It may even be drunk in the beginning of the sweating stage, though more sparingly. Its cautious use at this time will promote the flow of the sensible perspiration, which after it has commenced, seems often to be retarded by a fresh increase of animal heat. A draught of cold water taken under such circumstances, will often reduce the heat to the standard at which perspiration flows more freely, and thus bring the paroxysm to a speedier issue.

3. But after the sensible perspiration has become

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* *See Alpinus. Med. Meth. lib.* ii. *cap.* 3. After reciting the effects of cold drink in diminishing heat and thirst, and exciting profuse perspirations and large discharges of urine, he concludes, *Mirabile est, quomodo tale praesidium, has febres expugnat; nam execretionibus, quas aqua suscitat, hae febres finiunter.* According to the same author, this practice was followed by the Egyptians. *See lib.* ii. *cap.* 15.

The

come general and profuse, the use of cold drink is strictly to be forbidden. At this time I have perceived in more than one instance, an inconsiderate draught of cold water, produce a sudden chilliness both on the surface and at the stomach, with great sense of debility, and much oppression and irregularity of respiration. At such times, on applying the thermometer to the surface, the heat has been found suddenly and greatly reduced. The proper remedy is, to apply a bladder filled with water, heated from 110° to 120°, to the scrobiculus cordis, and to administer small and frequent doses of tincture of opium, as recommended by Dr. Rush. By these means the heat is speedily restored.

This effect of cold water used as a drink during profuse perspiration, is precisely analogous to the affusion of it at such times on the surface of the body, a practice known to be of the utmost danger, and enumerated by Hoffman among the causes of sudden death. *Inveniuntur in amplissimo regno naturæ plura quæ brevi tempore nocent ac perimunt, ut aqua frigida corpori sudore diffluenti immoderatius superingesta. Vol.* i. *p.* 194.

The

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The pernicious effects of cold drink during profuse perspirations was known to the ancients.—*See Celsus, lib.* i. *cap.* 3.

The pernicious effects of cold water applied internally and externally during profuse perspiration, depend on the same causes, namely, that perspiration itself is a cooling process, under which when profuse, the heat of the body, whatever its actual state may be, is sinking; that under such circumstances, we find as a matter of fact, that it parts with its remaining heat more easily; and on the sudden application of cold, that this heat sinks to a degree which disturbs, and sometimes wholly interrupts the actions on which life immediately depends.

Thus then we may safely adopt the same general rules for the use of cold water in fever as a drink, that have already been laid down for its external application. It may be used as a drink, in fevers, at any time, *when there is no sense of chilliness present, when the heat of the surface is steadily above what is natural, and when there is no general or profuse sensible perspiration.**

Though we have arrived at these conclusions, the effects of cold water used as a drink have not been inferred from its external application, but made the subject of separate inquiry. Yet, that in the one case, and in the other, they should be similar

* See p. 17.

similar in kind, though different in degree, will be expected by every one acquainted with the laws of the animal œconomy, and particularly with the sympathy that subsists between the stomach and the surface.

I have only to add, that in our common contagious fever, when I have used the affusion of cold water, I have seldom found it necessary to employ it largely as a drink, and my experience of its effects when drunk in large quantities, has been chiefly confined to those cases, where the fears or prejudices of the patients, or their friends, have prevented our having recourse to the more powerful method of affusion. For, however burning the thirst may be, it is speedily abated, and even removed, with very little drink, and often without any, by the successful use of the affusion on the surface. Though the affusion in general suffices in *our* contagious fever, yet where cold water is employed in the dreadful fever of Philadelphia and the West Indies, it is probable, that its internal and external use should be combined; a point that must be determined by the actual heat of the patients, measured by the thermometer, and by their sensation of heat; circumstances of which it is to be regretted that we have as yet no accurate information.

CHAP.

# CHAP. XII.

*Of the disease that arises from drinking cold liquids, or using the cold bath, after severe exercise.*

IT is here natural to inquire how far the fatal effects proceeding from drinking cold water, not in fever, but in cases where the system has been extremely heated by bodily exertions (of which the records of medicine afford so many instances) are to be explained on the principles already laid down. If they are explicable on these principles, we ought to be able to shew, that they have occurred in situations where the system, after having been much heated and enfeebled by severe exertions, is losing its preternatural heat from profuse sweating, and in general also from the cessation of the exertions by which this heat was originally produced. Here two powerful causes combine to cool the body, and if under their operation, a sudden application of cold is made either to the

stomach or the surface, the living power will, we know, resist it faintly, and the fatal consequences be accounted for.

1. In my own experience this sudden death has occurred once only, and that many years ago. It was in the case of a young man who had been engaged a long time in a most severe match at fives. After it was over, he sat down on the ground, panting for breath, and covered with profuse perspiration. In this state he called to a servant to bring him a pitcher of cold water just drawn from a pump in sight. He held it in his hand for some minutes, but put it to his head as soon as he had recovered his breath, and drank a large quantity at once. He laid his hand on his stomach, and bent forwards; his countenance became pale, his breath laborious, and in a few minutes he expired. Various methods were employed to restore him, but in vain.

2. The following case resembles very exactly that just given. *Blasius, Senensis, familiaris noster et condiscipulus, dum longiusculo tempore sub ardentissimo sole pilæ lusu incaluisset, nec* sudore adhuc aut fatigatione remissis, *in subterraneum locum ubi vinaria erat cellula, descendisset, frigidissimi vini calicem hausit; quo epoto, statim defecit.* Benevent. cap. 17. De abditis.

3. *Elegans*

3. *Elegans & optimæ staturæ juvenis Romanus, cum pilâ luderet,* et sudore respersus, ac totus madidus, & fatigatus *ad puteum, pro siti arcendâ venisset, exhaustâ frigidâ recens per caldarium extractâ, illico in terram cecidit & obiit.*

4. *Alterum novimus ex iis, qui in campo negotiantur, qui quum non minus corporis totius* adapertis poris madidus *domum reverteretur, cyathum frigidioris aquæ ebibit, et mortuus fuit.* Anat. Lusit. curat. med. cent. 2. curat. 62.

5. Forrestus relates, that in the year 1544, Valerius Cordus, a young man of great learning and talents, went during the heat of the dog-days, to collect plants among the Florentine mountains. Exhausted with fatigue and thirst, he incautiously drank of a cold spring which issued out of one of the hills, and was immediately seized with a fever, of which he died; but the symptoms of which he has not recorded. *P. Forestus, lib.* i. *Scholio ad obs.* 13.

6. Scaliger relates the case of a reaper, who, stooping down to drink at a fountain after severe labour, instantly expired. *Scaliger de Subt. ad Cardan. exer.* 13.

7. In Heister's observations, a case is related of

a young man, who, about the Christmas season, had been playing and dancing at a Mill with some young women, and had eaten greedily of some hot buttered cake. After this, being extremely thirsty, he took a large draught of some cold water mixed with snow. An inflammation in the stomach followed, terminating in mortification, of which he died. *Heister's Med. &c. Observations, translated by Wiseman, p.* 17.

8. *Villanum quendam nobis familiarem novimus, qui messis tempore, anno,* 1597, *exhaustus viribus, et totus sitibundus præ nimio solis ardore, domum rediens, cum in magna quantitate, ad sitim explendam, gelidam (aquam) bibisset, exanimatus mox juxta puteum cecidit, ac intra tres horas animam expiravit.* Georgius Grasseccius in Theatro Anatomico.

These relations are chiefly taken from the collection of Schenck—they might be greatly enlarged, and I have collected a number of similar cases; which it were a tedious and an useless task to detail. In all of the cases which I have consulted, as well as in those I have related, three circumstances are either expressed or may be clearly inferred—1. The body had been previously heated beyond the temperature of health, by exercise carried to fatigue. 2. To this violent exertion

exertion a state of rest had succeeded. 3. A profuse perspiration had taken place. So far our reasoning is supported; but as these points are of the utmost consequence in explaining the operation of cold on the human body, and as direct experiments are attended with extreme hazard, the reader will excuse me, if I attempt to illustrate them by such evidence as history may incidentally afford.

1. In Quintus Curtius *(lib.* vii. *cap.* 5), an account is given of the march of the army of Alexander the Great in pursuit of Bessus, through the country of the Sogdiani, which is represented as destitute of water, sterile, and covered with scorching sands. The intolerable heat, fatigue, and thirst of the soldiers, in their march through this burning desert, are described with all the florid eloquence of the historian. At length, fainting under their toils, they reached the banks of the river Oxus, where by indulging large draughts of the stream, Alexander lost a greater number of his troops than in any of his battles. *Sed qui intemperantius hauserant intercluso spiritu extincti sunt; multoque major horum numerus fuit, quam ullo amiserat prælio.**

1. A similar

* The whole particulars of this march, as described by Quintus Curtius, are very interesting. The desert, which

2. A similar story is related by Appian—*Appianus Alexandrinus de bellis civilibus, lib.* v. *tradidit Cornificianos milites a Pompeianis pugna fatigatos & æstuantes fontanam aquam avidè bibentes*

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contained not a drop of water, was four hundred Stadia across—*(per quadringenta stadia ne modicus quidem humor exsistit)* that is, upwards of forty-six English miles. They began their journey in the night, directing their course by the stars, and for some time their march was tolerable, being refreshed by the dews of the night, and the coolness of the dawn; but when the sun rose, the heat became troublesome, and as the day proceeded, most oppressive; it was equally painful to stand still or proceed. After a day of dreadful fatigue, the vanguard of the army, and Alexander himself, reached the Oxus towards evening; and such as were themselves refreshed, were employed in carrying water back to the fainting troops behind. As they arrived in succession on the banks of the river, it may easily be supposed that they drank without moderation, and hence the destruction that ensued. It was on this occasion that Alexander displayed his magnanimity, in refusing the cup of water brought to him as he advanced, because it was not sufficient both for him and his companions; and that he gave a proof of his genius, by ordering fires to be kindled on the high banks of the Oxus, not merely to direct his way-worn soldiers through the darkness, but to animate their fainting exertions by a prospect of the end of their toils. Plutarch alludes to this story, but does not relate it fully. I fear it is not to be found in Arrian. A similar distress is mentioned by him (*lib.* vi. *p.* 425.) to have occurred in Alexander's march through the deserts of the country of the Gedrosi, who inhabited the southern part of the Persian empire, on the shores of the Indian ocean.

*bentes, ex iis plurimos emortuos. Marcel. Donat. lib.* iv. *cap.* 6. *Hist. med. mirab.*

3. A disaster of the same kind is recorded to have occurred to the Christian army in the holy wars. *Gulielmus Tyrius, lib.* iii. *c.* 16. *scribit, Christianum agmen Pisidiam ingressum, regionem arentem & inaquosam, tandem invento fluvio, avidè bibisse; quod quidem, qui largius aquam frigidam ingurgitaverunt, sitis discrimen evadentes, mortem in aquarum opulentiâ reperêre.*

These historical relations support very fully the doctrine I have already laid down. The cautious reasoner may not, on a hasty consideration, be inclined to rest with much confidence on this sort of evidence; but on reflection he will see that it is entitled to considerable authority; because the facts are in their nature not liable to be mistaken, and because they are not likely to be misrepresented. It is also entitled to much weight, because it is not given by these historians in support of any particular doctrine; and because the experiment having been made on such numbers of persons at once, it acquires an authority hardly to be ascribed to solitary cases, however accurately detailed. These considerations induce me to lay much more stress on evidence of this kind, than on the precepts respecting the effects of cold drink to be found

found in medical authors, ancient or modern. Nevertheless I have looked into the greater part of the ancients on this point, (for among the moderns there is not much on the subject), and have found nothing, that fully considered, invalidates the conclusions I have laid down.

Of the ancient physicians, the most copious on the use of water, in all its forms, is Galen. He not only used cold drink, but immersion in the cold bath, in burning fevers, with extraordinary success. His relations appear to me, in general tedious and obscure, but not destitute of truth; and the weariness of perusing him, is occasionally relieved by the pleasure of rescuing a fact that was buried under masses of false theory. The reader who would consult him on this subject may use the references below.*

In the first volume of *Medical Inquiries* and *Observations*, published by Dr. Rush of Philadelphia, 1789, an account is given of the "Disor-"der occasioned by drinking cold water in warm "weather,"

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* Vol. i. 23. B.
Vol. ii. 78. C.
Vol. vii. 70. A. and forwards throughout the volume. I quote from the Latin edition in folio, published at Venice, 1656.

"weather," which frequently occurs there, "Three "circumstances," he observes, "generally concur "to produce disease or death from drinking cold "water. 1. The patient is extremely warm. 2. "The water is extremely cold. And, 3. A large "quantity of it is suddenly taken into the body. "The danger from drinking cold water is always "in proportion to the degrees of combination "which occur in the three circumstances that have "been mentioned." *p.* 151. Dr. Rush goes on to state the symptoms of this disease, which are, I apprehend, given with accuracy. His method of cure my experience neither authorizes me to confirm nor oppose. "I know but one certain remedy "for this disease, and that is *liquid laudanum*. "The doses of it, as in other cases of spasm, should "be proportioned to the violence of the disease. "From a tea-spoonful, to near a table-spoonful, "has been given in some instances before relief has "been obtained. Where the powers of life ap- "pear to be suddenly suspended, the same reme- "dies should be used which have been so success- "fully employed in recovering persons supposed "to be dead from drowning." To this I would add the application of a bladder filled with water, heated to 110° or 115° of Fah^t^. to the pit of the stomach, from which I have seen powerful effects in restoring the vital heat. But while I do not dissent

dissent from Dr. Rush's practice, I cannot subscribe to his notion of the causes of this disease, or to the method of prevention founded on this notion.

Dr. Rush seems to entertain the popular opinion on this subject: *the body is extremely warm, the water is extremely cold, and a large quantity is introduced suddenly.* He apprehends the danger to arise from the great difference between the temperature of the body, and of the water taken in. As a means of prevention he therefore proposes to such as cannot be restrained from drinking cold water when preternaturally heated— 1. To grasp the vessel out of which they are about to drink for a minute or longer with both hands, that a portion of heat may be abstracted from the body and imparted to the cold liquor. 2. If they are not furnished with a vessel to drink out of, but obliged to drink at a pump or a spring, always to wash their hands and face previously to drinking, with a little of the cold water. "By receiving," says he, "the shock of the water first on those "parts of the body, a portion of its heat is con- "veyed away, and the vital parts are defended "from the action of the cold."

The fact however is in my mind perfectly established,

blished, that there is no situation in which the application of cold to the body, whether to the surface or the stomach, is so safe, or in general so salutary, as when the heat of the body, from whatever cause, is preternaturally great, provided that the body is not already in a state in which it is rapidly parting with this heat, and no disease has taken place in the general sensibility, or in the structure of any of the parts; and that where the body is preternaturally heated, the degree to which cold water may be drunk, may be always decided by the steadiness of the sensation of heat, and the tenacity with which the preternatural heat is actually retained. Thus, in continued fevers it may be drunk to a greater extent, than in the hot stage of intermittents, because the heat is more firmly retained; the profuse perspiration not being at hand, by which the febrile heat of intermittents is carried off. The ancients who gave cold drink largely in continued fevers, were doubtful of its use in intermittents.* It may however be given (as

* See Sennertus, lib. ii. cap. ix. p. 54. Itaque Græci auctores jubent in statu, cum febris acuta, sitis, inquietudo, cordis & arteriarum pulsatio est vehementissima, ægerque avidissime eam expetit, aquam frigidam copiose exhibere.—

* * *

In intermittentibus febribus vero aquæ frigidæ potus nunquam convenit.

(as I have already stated) with great safety, in intermittents, provided it be taken in the time that intervenes after the hot stage of the paroxysm is fairly established, and before the sweat that follows it, has become general and profuse. As however it is only in that interval that it can be given in intermittents with advantage or safety, we can easily understand, that the ill effects arising from its being accidentally drunk in the cold, or the sweating state of the paroxysm, (in both of which the thirst often demands liquids) may have produced the doubts which some have expressed in regard to its use in intermittents, and the interdiction which in such cases others have pronounced against it. We may explain also from the same considerations, why in the accounts that have been handed down to us of injurious effects from the use of cold drink in fevers, the greater part of the cases have been intermittents.

The instances however that are recorded of the fatal effects of large draughts of cold liquids, have more frequently occurred after severe exercise and fatigue, than even in intermittent fever. The cause of this is obvious—the heat preternaturally accumulated by exercise, is held with less tenacity than even the heat in intermittents. It is dissipated by the perspirations that exercise occasions, and is speedily lost, when to profuse perspiration,

spiration is added a state of rest. It is then that a large draught of cold liquid is especially dangerous. But while the preternatural heat is sustained by continued exertion, cold liquids may be taken in moderate quantities without producing any injurious effects. They may even, I apprehend, be drunk copiously without producing suddenly the fatal effects already described—but in copious draughts, they are found oppressive to the stomach during exercise, and excite languor, nausea, and sometimes vomiting, as I have had occasion to observe. In the narrative already mentioned of the march of Alexander's army through the desert country of the Sogdiani, it is related by the historian, that a few of the soldiers, by the advice of the natives, had provided themselves with water, of which under their burning thirst they drank immoderately. The consequence was, that they became heavy, feeble, and unable to support their arms, and this state of oppression was succeeded by severe vomiting. *Graves deinde avide hausto humore, non sustinere arma; non ingredi poterant; et feliciores videbantur, quos aqua defecerat, quum ipsi sine modo infusam vomitu cogerentur egerere. Q. Curtius, lib.* vii. *cap.* 5. The water thus wasted, or worse than wasted, might have been used to advantage in wetting, from time to time, the garment next the skin. Thus the oppression of its weight on the stomach

stomach would have been prevented, and the surface of the body being kept cool by constant evaporation, the heat of the system would have been moderated, and the thirst alleviated.*

If this account of the circumstances under which cold drink after severe exercise proves injurious, be just, the directions of Dr. Rush to those who *will* drink in such circumstances, are founded on error. By abstracting a part of the preternatural heat of the body before drinking, the danger is not diminished, but greatly increased. This enlightened physician will excuse these observations, drawn from me by a consideration of the importance of the subject, but accompanied by sentiments of sincere esteem and respect.

If

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* Postillions understand the difference between giving their horses cold water to drink, during exercise, and after the exercise has ceased. When in their power, they always water their horses two or three miles before the end of their journey. My friend, Mr. Charles Aikin, assures me, that during his tour on foot through Wales, in company with his brother, of which Mr. Arthur Aikin has given so interesting and useful an account, they drank of the pure streams, as they descended from the mountains, without reserve, during the fervor of the day, taking care however never to rest after drinking.

If the effects of cold water used internally under severe exercise are not entirely analogous to the effects produced by its affusion on the skin;—the difference will be easily understood, by those who consider, that where a quantity of water is swallowed, besides the influence of the cold, the stomach sustains a load, from the weight and the bulk of the liquid, particularly oppressive under the constant action and agitation of the voluntary muscles, from which the surface, moistened with water, is entirely free; and on the other hand, that the evaporation from the surface, promoted by the immediate access of the external air, must operate more directly in cooling the body, and particularly in counteracting the burning rays of the sun, than water taken into the stomach. With these exceptions, the operation of cold liquids on the stomach and on the surface of the body are analogous in the case of preternatural heat produced by bodily exertion, as in all other cases of preternatural heat. As it is safe to drink cold water in proportion as the heat from exercise is great and steady, so also is it safe, according to this ratio, to pour it on the surface, or to immerse the body in the cold bath.

In the earlier stages of exercise, before profuse perspiration has dissipated the heat, and fatigue debilitated the living power, nothing is more safe, according

according to my experience, than the cold bath. This is so true, that I have for some years constantly directed infirm persons to use such a degree of exercise before immersion, as may produce some increased action of the vascular system, with some increase of heat; and thus secure a force of re-action under the shock, which otherwise might not always take place. The popular opinion, that it is safest to go perfectly cool into the water, is founded on erroneous notions, and is sometimes productive of injurious consequences. Thus, persons heated and beginning to sweat, often think it necessary to wait on the edge of the bath until they are perfectly cooled; and then, plunging into the water, feel a sudden chilliness that is alarming and dangerous. In such cases the injury is generally imputed to going into the water too warm, whereas in truth it arises from going in too cold.*

But

* Dr. A. Munro Drummond, in his inaugural dissertation, "*De Febribus Arcendis*," the only specimen left of his admirable talents, speaking of the effects of the cold bath as a preventive against the action of contagion, observes, "Nec "frigida protinus fugienda vel calida temperanda, quamvis "cutis pallida aliquantisper siat vel leviter aliquis inhorruerit. "Si exercitatio antecedat quæ citra lassitudinem & sudorem "est, hæc facile evitari posse experiendo didici: et fere, si "nil aliud obstet, quo ante quisque plus incalueret quam "aquam intraverit, eo calidior emerget."

But though it be perfectly safe to go into the cold bath in the earlier stages of exercise, nothing is more dangerous than this practice after exercise has produced profuse sweating, and terminated in languor and fatigue. Because, as has already been repeated more than once, in such circumstances, the heat is not only sinking rapidly, but the system parts more easily with the portion that remains.

This account of the operation of the cold bath, will explain some circumstances very generally mentioned by writers on the effects of cold on the human body, and hitherto not properly accounted for. That the Roman youth, in the heat of their exercise in the *Campus Martius*, frequently plunged into the Tiber, is a fact universally known; they found in this practice a high enjoyment, and they believed it conducive to health, and more especially to sleep. On the other hand, various relations may be cited of the injurious effects of an apparently similar practice; the most remarkable of which is the case of Alexander the Great, when covered with dust and sweat, he threw himself into the Cydnus, and was seized with a disease, of which he nearly perished; one of the best authenticated facts in ancient history.

In the dissertation *De febribus arcendis*, by Dr.

A. Munro Drummond, these facts are brought together in the following words. *Alexander, quondam diei fervidissimo tempore, liquore fluminis invitatus, vix dum in Cydnum amnem descenderat, cum gravi inde morbo implicitus est. Romana juventus, post quotidianas in Campo Martio exercitationes, pulvere simul & sudore perfusa, Tyberi impune lassitudinem cursus natandi labore deposuit. Mali nimirum adsuetudine duramur in his sicut in cæteris rebus omnibus. Thes. Med. vol.* iii. *p.* 154. Doubtless the influence of habit has a considerable share in regulating the effects of cold on the human body; but the circumstances just mentioned, seem capable of an explanation on other principles.

On the *Campus Martius*, the exercises of the Roman youth were carried on with all the vehemence of emulation. Swimming formed a part of those exercises*, and generally terminated the foot-race. The youthful candidates in this exercise directed their course towards the banks of the

* See *Horace, lib.* i. *ode* 8. Where the poet, after questioning Lydia why her lover neglects his accustomed exercises on the *Campus Martius*, among other particulars asks,

*Cur timet flavum Tiberim tangere?*

See *Vigetius, lib.* i. *cap.* x.

See also *Hieron. Mercur. lib.* iii. *cap.* 14.

river, and plunged headlong into the stream. Sometimes the contention did not terminate till they had swum across the river twice. Hence it will easily be seen, that they were accustomed to immerse themselves in water in the very fervor of their exertions, when the heat was preternaturally great; and not after the body was cooled by profuse perspirations, or exhausted by long continued fatigue. In this situation, the practice was safe; without taking into consideration, that the persons following it, were in the flower of life, fortified by early habits, and partly defended from the shock of immersion by the inunctions which seem to have been generally used among the Romans, before the cold*, and after the hot bath;



* See *Horace, Satyr*: *lib.* ii. *sat.* 1.

——— *Ter uncti*
*Transnanto Tiberim, sommo quibus est opus alto.*

The subject of the ancient unguents is treated of by *Hieronymus Mercurialis, de arte Gymnastica, lib.* i. *cap.* x.

The same author (*lib.* iii. *cap.* 14.), speaking of swimming, so much in estimation among the ancients, observes, *quo pacto vero similis exercitatio perageretur, tanquam res vulgatissima silentio praeterita fuit fere ab omnibus, uno excepto Antyllo, qui natalturos prius moderatè unxisse, et frictione corpora praecalfecisse, deinde e vestigio in aquam se projicere debere voluit, quasi omnes sanitatis gratia natantes eas regulas observarent.*

This

and which were particularly employed by the *athletæ* of Greece and Rome in all their exercises. It was the more safe, because the stream of the *yellow Tiber*, being comparatively scanty and slow, its waters speedily received the influence of the sun, and acquired the temperature of the atmosphere. Where the air and the water are of the same temperature, the rarer element prepares the body for the contact of that which is more dense.

The circumstances under which Alexander plunged into the Cydnus, were different in many essential points. He had marched at the head of his troops to seize a pass in Mount Taurus, necessary to facilitate his passage into Cilicia. After having secured his object, he descended from his elevated station, through a road, difficult and full of defiles, to the city of Tarsus, which was situated at the bottom of the mountains. His whole march, which probably continued several days, was attended by extraordinary exertion, not from the nature of the country only, but also from the pressure of circumstances.

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This method of previously anointing the body, might be recommended to our modern swimmers, not only as defending them from the shock of immersion, but as enabling them to glide through the water with less effort.

In ascending the heights, he had to hasten forwards, lest the enemy should pre-occupy the pass by which Cilicia must be entered. In descending from the immense elevation he had reached, he was stimulated by the desire of saving the city of Tarsus, the capital of the province, which the Persians threatened to consume by fire. In both of these enterprises Alexander was successful. It was at the close of this extraordinary march, according to the testimony of all the historians, that the conqueror, advancing into Tarsus at the head of his troops, covered with dust and sweat, and exhausted with long continued toil, stripped himself in the sight of his army, and plunged into the pure and cold waters of the Cydnus, which ran through the city. The symptoms that followed, are described with sufficient minuteness and precision. *Vixque ingressi subito horrore artus rigere cœperunt; pallor deinde suffusus est, et totum propemodum corpus vitalis calor reliquit. Exspiranti similem ministri manu excipiunt, nec satis compotem mentis in tabernaculum deferunt.* *Q. Curtii, lib.* iii. *cap.* v. From the length and difficulty of the march, it is natural to suppose, that Alexander must have been cooled, as well as debilitated, by excessive perspiration and fatigue, and under such circumstances, immersion in the cold and rapid Cydnus, was followed by the consequences which

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we should expect from the principles already laid down*.

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* If an objection be urged against our laying much weight on a relation taken from a period of history so remote, I reply, that I am far from resting any part of my conclusions upon it, having the firmer foundation of actual observation. But as this adventure of Alexander is mentioned by almost every writer on the effects of cold on the living system, to exemplify the danger of its being applied when the body is preternaturally heated, it seemed proper to shew, that it is capable of a more just and natural explanation.

In regard however to the fact itself, of Alexander having suffered from bathing in the Cydnus, it might be easily shewn, that we cannot reject it without resisting entirely the weight of historical evidence. 1. This exploit was performed before thousands of witnesses, by some of whom it is recorded, and it is mentioned by all the historians. 2. The circumstances that preceded it, and the consequences that followed it, are also recorded with great minuteness, and form a natural chain of events. 3. The act itself of stripping before his army, and springing into the Cydnus, may have an air of improbability; but is not so inconsistent with Greek, as with modern manners; and however unlike the dignity of great commanders in our days, is perfectly characteristic of Alexander —of that ardent and daring temper, and of that buoyant imagination, which were equally regardless of danger and decorum; a temperament, that joined to extraordinary talents, attached his soldiers to him in so singular a degree, and which in the æra of the world in which he lived, was calculated to carry him to the astonishing elevation which he attained. 4. The exploit itself was of a nature not likely to be feigned; and the issue of it had nothing marvellous—there

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In his essay on swimming, Franklin makes the following observation: "During the great heats "of

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was nothing in any part of it, by which the son of Ammon, or his followers, could be in any way gratified.

The effects of Alexander's bathing in the Cydnus, gave a celebrity to this river which it otherwise would not have acquired. I have called it cold and rapid, meaning to speak of it comparatively with the Tiber. The Cydnus is one of the principal streams that descend from Mount Taurus, (*Strab.* xiv. *p.* 462.) at the bottom of which stood the city of Tarsus. From the great elevation of the mountain, the Cydnus must in this situation be rapid and cold; and colder because rapid. From the same cause it would be easy to shew (if it were at all to the purpose) that the transparency ascribed to its waters was probably ascribed justly. Quintus Curtius imputes its coldness to the rays of the sun being excluded by the shadiness of its banks: the more accurate and philosophic Arrian, mentions as the cause, the loftiness of the mountains whence its fountains descended. Quintus Curtius speaks of it as gliding through a gentle descent, *(leni tractu è fontibus labens)* and Tibullus of its silent and placid stream.

> *At te, Cydne, canam, tacitis qui leniter undis,*
> *Cæruleus placidis per vada serpis aquis.*

Strabo, on the other hand (*lib.* xiv. 392.), mentions its current as most rapid, and this to some of the scholiasts seems a contradiction. It is not so in reality. If the declivity was uniform, the current might be at once swift and silent. The river Moffat (the source of the Annan) descends from the mountain of Hartfell, by a declination, which in three thou-

sand

" of summer there is no danger in bathing, how-
" ever warm we may be, in rivers which have
" been thoroughly warmed by the sun. But to
" throw ourselves into cold spring water, when
" the body has been heated by exercise in the
" sun, is an imprudence which may prove fatal.
" I once knew an instance of four young men,
" who having worked at harvest in the heat of
" the day, with a view of refreshing themselves,
" plunged into a spring of cold water; two died
" on the spot, a third the next morning, and the
" fourth recovered with great difficulty. A co-
" pious draught of cold water in the same cir-
" cumstances is often attended with the same
" effect in North America." The authority of the American Bacon is of great weight in medicine, as in every other branch of science that he touches; and particularly in what respects immersion in water; for doubtless he spent more time

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sand yards, amounts to two hundred and thirty feet, with such uniformity, that in the course of some miles there is hardly a single ripple. To finish a digression by far too long, but which may relieve the dryness of a professional subject, it may be observed, that our account of the illness of Alexander, receives confirmation from a difference among the historians respecting its causes; some imputing it to his having bathed in the Cydnus, and others to his previous fatigue: at the distance of two thousand two hundred years, it may be clearly determined, that both circumstances combined in producing his disease.

time in this element than any philosopher of modern days. It may however be easily supposed, that he adopted the commonly received opinion that the injury arose from the persons in question going in *when hot*, instead of from going in *when cooling, after having been heated;* to which latter circumstance, it can hardly be doubted, that the fatal accident he relates, was to be imputed. It is not however to be denied, that the difference between the temperature of spring and river water, at certain seasons of the year, will make a great difference (in any circumstances of the body) in the consequences of immersion. Springs which issue copiously from the earth, are, as we know, both from reasoning and observation, nearly of the temperature of the earth at some distance under the surface; and this temperature, when unaffected by chemical changes, is probably (for actual experiment is not yet sufficiently extensive to affirm the proposition universally) of the mean heat of the atmosphere, taking the year round, under which it lies*. On the other hand, rivers, even of the largest size, acquire at a certain distance from their source, the temperature of the earth, and rise towards the end of summer, to nearly the highest degree of the summer heat.

* See Dr. John Hunter's valuable paper on this subject, in the Philosophical Transactions for 1788, *p.* 53.

I shall close my remarks on the circumstances which render the cold bath safe or hazardous after exercise, with the following narrative:—

On the first of September, 1778, two students of medicine at Edinburgh set out on foot on a journey, a considerable part of which lay along one of the rivers of Scotland. They started by sun-rise, and proceeded with alacrity in the cool of the morning. At the end of eight miles, they breakfasted, rested for an hour, and then resumed their journey. The day grew warm as it advanced, and after a march of eight miles more, they arrived heated, but not fatigued, on the banks of the river above-mentioned, about eleven in the forenoon.—Urged by the fervor of the day, and

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"to the *quality* of these waters, but to the *warmth*, in "consequence of their long exposure to the sun." There is no doubt of the justice of this observation. The body of water which descends the bed of the Mississippi, immense as it is, running for a thousand miles and upwards, in a channel, not perhaps at its upper end one hundred yards above the surface of the sea, and in the whole of its course exposed to the rays of the sun, must acquire the temperature of the atmosphere; exhibiting however in the middle of its stream, rather the average heat of the atmosphere for the three or four preceding days, than at the moment of observation. The same may be said of the waters of the Ganges, of the Nile, and of other great rivers.

and tempted by the beauty of the stream, they stripped instantly, and threw themselves into the river.—The utmost refreshment followed, and when they retired to the neighbouring inn, this was succeeded by a disposition to sleep, which they indulged. In the afternoon they proceeded, and travelling sixteen miles farther at a single stretch, arrived at the inn where they were to sleep, a little after sun-set.—The afternoon had been warm, and they sweated profusely; but the evening was temperate and rather cool. They had travelled for some miles slowly, and arrived at the end of their journey, stiffened and wearied with their exercise.

The refreshment which they had experienced in the morning from bathing, induced however one of them to repeat the experiment, and he went perfectly cool into the same river, expecting to relax his limbs in the water, and afterwards to enjoy profound sleep. The consequences were very different. The Tweed, which was so refreshing in the morning, now felt extremely cold; and he left the water hastily. No genial glow succeeded, but a feverish chill remained for some time, with small frequent pulse, and flying pains over the body. Warm liquids and friction brought on at length considerable heat, and towards morning perspiration and sleep followed.

Next day about noon they proceeded on foot, but the traveller who had bathed was extremely feeble; and though they had to perform a journey of a single stage only, as some part of it was difficult and mountainous, he was obliged to take the assistance of a carriage which overtook them on the road. It was several days before he recovered his usual vigour. This relation will not I hope be deemed of the less authority, because it is given by the person who suffered by his imprudence.* It is unnecessary to point out the application of these incidents to the doctrines already laid down.

It seems to be a general truth, that from whatever cause the heat of the body is increased, in proportion to this increase (provided no local disease has occurred) is the safety with which cold may be applied. In the celebrated experiments

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* Those who know the road from Edinburgh to Moffat, will be at no loss to mark the different stages of this adventure.

I may here observe, that illnesses similar to the above occur frequently, from the imprudence of the country people, (the Boltoners, as they are called) who make an annual visit to the river below Liverpool, for the purpose of bathing.—In such cases, I have found the disease that followed, was not inflammatory, as might be supposed, but rather a fever of debility, as in the instance detailed.

ments of Dr. Fordyce, Sir Charles Blagden, and others, of which an account is given in the Transactions of the Royal Society, *vol.* lxv. *p.* 111, and 484, it is repeatedly mentioned, that these gentlemen passed from a room heated to 200°, and upwards, into the cold air, with perfect safety. "During the whole day," says Sir Charles Blagden, "we passed out of the heated "room," (where the temperature of the air seems to have been 240°, and sometimes 260,) "after "every experiment, immediately into the cold "air, without any precaution; after exposing "our naked bodies to the heat, and sweating "most violently, we instantly went into a cold "room, and staid there, even some minutes, be-"fore we began to dress; yet no one received the "least injury." *Ibid. p.* 494.

The freedom from injury, and even from inconvenience, which these gentlemen experienced, depended on the increased heat of the body, and the increased action of the arterial system.—Had they continued exposed naked to the cold air till the heat sunk as low as its natural standard, and the heart and arteries subsided into their usual state of action, their situation would have been very hazardous.

In similar experiments, repeated at Liverpool, of

of which a detail is given by Dr. Dobson, in the same volume of the Philosophical Transactions, *p.* 463, the gentlemen engaged in them passed from the heated room into the cold air, with equal impunity. My friend Mr. Park assures me, that after remaining some time in the stove, where the heat was as high as 202°, he went into the external air without a great coat, or any other than his usual clothing, during a hard frost, and perceived neither injury nor inconvenience.

In many manufactures, the persons employed are exposed to extraordinary degrees of heat, particularly in the manufacture of glass. Such persons, guided by nature, often endeavour to obtain relief, by exposing themselves to degrees of cold, which on the commonly received opinions, would in their situation appear extremely dangerous. Thus at Glasgow, in the spring of the year 1780, I learnt, that it was common for the workmen in the glass manufactory, after enduring for some time the consuming heat of their furnaces, to plunge into the Clyde; a practice which they found in no respect injurious.

A great variety of the phenomena respecting the influence of cold on the living body, receive an easy explanation by attending to the principle already

already mentioned. Thus, after the heat of the body is increased two or three degrees in the hot bath, it is not only safe, but refreshing, to plunge into the cold bath, as I have repeatedly experienced. A practice of this kind prevails, as is well known, in Russia, where it is common, after remaining some time in the hot bath, to roll naked in the snow, and return to the warm bath as before. The Russian hot baths vary from 106½° to 116° of Fah'.* and I find by experiment, that the actual heat of the living body is increased in the hot bath, unless under particular circumstances afterwards to be explained, when the temperature is no higher than 100°. We may therefore safely infer, that in making these singular transitions, the heat of the Russian is in the first instance increased beyond the natural standard; and it is to this increase that the safety, as well as the agreeableness of the practice, is to be attributed; and from all these facts we may conclude, that, where the actual heat is considerably increased,

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* See *Dissert. physica experimentalis de calore Animalium*, by *J. A. Braun*, in the *Nov. Comment. Academiæ Scientiarum Imperialis Petropolitanæ*, vol. xiii. The Abbé Chappe D'Auteroche says the Russian baths are as high as 60° of Reaumer, (160° of Fahrenheit) *Philosophical Transactions, vol.* lxv. *p.* 112; but he speaks of vapour baths.

and the body is not weakened by fatigue, even the presence of profuse sensible perspiration will not render exposure to a certain degree of cold dangerous.

It is in this way that we must account for the safety of a practice, which prevailed at Rome under the first of the emperors. After the overthrow of the republic, the Romans consoled themselves for the loss of their freedom, by a more unbounded indulgence than ever in those sensual gratifications which had led to their fall. Of these, the pleasures of the bath formed a distinguished part; they sought every means of heightening and diversifying them, and connected them with other epicurean enjoyments. The mild and tepid immersion of the Greeks no longer satisfied them; they heated their baths to the utmost pitch of endurance; and, as they rose, reeking from their surface, vessels full of cold water were dashed over their naked bodies, as a high gratification in itself, and a means of stimulating the senses to gratifications still higher. Such practices could not however be continued without injury. In the bagnios of *Imperial* Rome, the last of the Romans, the followers of Zeno and Cato, were melted down into slaves; the powers of their minds became enfeebled, the vigour of their frames

frames decayed, and they lost for ever the bold impressions of freedom and of virtue.*

It was supposed by Dr. Cullen, who paid much



* This subject might lead to digressions, which my limited plan does not admit. I shall content myself with a few observations. Though private baths were in use in Rome from the earliest periods of their history, yet the system of public hot baths did not, it is understood, commence till the days of Augustus. According to Dion, Mæcenas introduced it.—Agrippa followed his example.—It was soon carried to an astonishing height, and the construction of baths where the people might be accommodated *gratis*, was an established and a successful method of gaining their affections. The extraordinary expense and magnificence of those structures are well known—the remains of the baths of Caracalla and Dioclesian, testify their former grandeur in our own days. According to Fabricius, there were 856 public baths at Rome, and some of these were large enough to contain at once 1800 persons. The *rage* for hot bathing in Rome, under the first Emperors, exceeded all bounds.

In regard to the exact temperature of their baths, as the thermometer was not known to the ancients, we cannot speak precisely. We have however the authority of Seneca and Plutarch for saying, that the bath of the Greeks was of a moderate temperature ; and, as Alexander found refreshment in the bath, in the burning fever of which he died, it probably did not exceed 93°, or at most 96°, of Fahrenheit. But at Rome, in the days of Seneca, the hottest baths were most in estimation, and those of Nero seem to have exceeded

much attention to the operation of temperature on the living system, that in all sudden changes, from a higher to a lower temperature, a sensation of cold takes place, even though the lowest point of change be such as, when permanent, becomes oppressive by its heat. Thus, that a change of the external air from 90° to 85°, is accompanied by a sensation of cold, though this speedily goes off, and is succeeded by a sensation of heat, if the temperature of 85° be continued*. This observation ought, I think, to be restricted to degrees

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ceeded all others in heat.—We may infer this from one of the best of Martial's epigrams.

*Si temperari balneum cupis fervens,*
*Fauſtine, quod vix Julianus intraret,*
*Roga, lavetur, Rhetorem Sabinæum;*
*Neronianas is refrigeret thermas.* *Lib.* iii. 25.

A person was employed at one time to regulate the heat of the baths, but in Seneca's days this had fallen into disuse. (*Epist.* 37.) This rage for bathing was checked by Adrian, and regulated by Severus. The fashion of heating the baths to this extraordinary degree did not continue—We have the authority of Galen, who flourished soon after Seneca and Plutarch, that in his days very hot baths were no longer in use. *See Hier. Mercur. lib.* i. *cap.* 10.

* See *Dissert. Med. Inaug. De frigore*, by A. Cullen, published at Edinburgh, 1780, (*p.* 8 and 9) which contains the best general view of the doctrines of the author's celebrated father on the operations of cold.

degrees of heat inferior to that of the human body, for it does not seem true when applied to superior degrees of heat. If, for instance, a person is immersed in a bath heated to 104°, and a quantity of cold water be speedily admitted, so as to lower it to 98° degrees, a sensation of a very agreeable nature takes place, not however to be denominated cold.—Where the actual heat of the body has been raised, and the circulation accelerated, a much greater reduction of the temperature of the bath must take place before it becomes cold to the sensations. In the same manner a person under the steady heat of fever, (102° to 106°) suddenly throwing off his bed clothes, and exposing himself naked to the external air, does not perceive it cold, but merely cool, a sensation too which goes speedily off. It is true there are some exceptions to this in fever, of which I have given one instance in detail. *(See page* 46.)

The manner in which our sensations are affected by changes of temperature, is a subject of importance, as well as of difficulty, for without a more precise knowledge of this, the action of temperature on life cannot be understood. In the state of health, I believe, however, that our sensations of heat and cold may be reduced to general principles.—But in certain of the Neuroses, as well

well as of the Pyrexiæ, these sensations are much perverted, and even in health they are much influenced by constitution, habit, and situation, To enlarge on this point would however lead into the general subject of the operation of cold on the living system, for which I am not sufficiently prepared, and which I have studiously avoided.

CHAP.

# CHAP. XIII.

*Use of the cold Bath in convulsive Diseases—Substance of a Paper read before the Medical Society of London—Use of the cold Bath in Insanity—General Remarks.*

IT might now be expected that some observations should be offered on the operation of cold water applied to the surface, or received into the stomach, in the various cases that have been related; but besides, that it is advisable that the reader should form his conclusions in a great measure for himself, we shall be better prepared for such general reasonings as may suggest themselves, when we have observed the operation of the cold bath on convulsive affections, and on some kinds of mania.—That the reader may have the whole of what I have to offer in one view, I shall here insert

insert the substance of a paper read before the Medical Society of London, May the 10th, 1790, and published in the third volume of their memoirs.

I shall first speak of Tetanus. For this disease, so terrible in its progress, and so generally fatal in its issue, several new remedies have been proposed within the last thirty years, and each in its turn has had some share of public confidence. But opium, mercury, the cold bath, and wine, are those only, which seem entitled to any reputation, and of each, it has been my fortune to have had some experience. This experience I proceed to give with all the clearness and faithfulness in my power.

1. George Gardner, a soldier in the Staffordshire militia, was put under my care by his officers on the 20th of February, 1781. About a fortnight before, after severe dancing and hard drinking at a country wedding, in which he had been employed two days and nights, he fell suddenly into a fit, which lasted an hour and a half, during which his consciousness was abolished. On recovery, he was affected with slight twitchings, which gradually increased, and were afterwards followed by fixed spasmodic contractions in

in different parts of the body, but more affecting the left side than the right. He had, when I saw him, all the symptoms of tetanus. The head was pulled towards the left shoulder, the left corner of the mouth was drawn upwards, the eyes were hollow, the countenance pale and ghastly, the face and neck bedewed with a cold sweat; but his most distressing symptom was a violent pain under the ensiform cartilage, with a sudden interruption of his breathing every fourth or fifth inspiration, by a convulsive hiccup, accompanied by a violent contraction of the muscles of the abdomen and lower extremities. He felt on this occasion as if he had received an unexpected blow on the scrobiculus cordis. Before I saw him, he had been bled and vomited repeatedly, and had used the warm bath, not only without alleviation, but with aggravation of his complaints. The three first remedies mentioned were used here in succession, viz. opium, mercury, and the cold bath.

He first took a grain of opium every other hour, afterwards a grain every hour, and at last two grains every hour; but he grew worse and worse during the two days on which this course was continued. The spasms extended to the back and shoulders, the head was at times retracted,

tracted, and the muscles of the abdomen partook of the general affection. Being no longer able to swallow the pills, he took no medicine of any kind on the night of the 22d, in the course of which general convulsions came on, and returned once or twice in every hour. The tincture of opium (liquid laudanum) was now directed to be given, and an ounce of the quicksilver ointment to be rubbed in on each thigh. In twenty-four hours he took two ounces and a half of the tincture without sleep, or alleviation of pain. The dose being increased, in the next twenty-six hours he swallowed *five ounces and a half* of the laudanum, a quantity which, at that time was I believe unexampled. He lay now in a state of torpor. The rigidity of the spasms was indeed much lessened, and the general convulsions nearly gone; but the debility was extreme; a complete hemiplegia had supervened; the patient's eyes were fixed, and his speech faltering and unintelligible.

As this young soldier appeared on the utmost verge of life, it seemed no longer safe to continue the laudanum, which had relieved spasm only in so far as it had brought on general paralysis. Intermitting this medicine therefore, we gave small doses of camphor from time to time in a liquid form, but the chief attention was directed to supporting

porting the strength by such nourishment as could be swallowed. Gruel, with a small quantity of wine, was ordered for him, though with much caution; for at that time I was not instructed with what safety and efficacy this last article might have been administered. For the next six days he seemed to revive: the general convulsions kept off, though the twitchings and convulsive hiccup continued. But on the night of the first of March he was seized, during sleep, with a convulsion as severe as ever, and this was followed by a return of all his symptoms with their former violence. The jaws were indeed more completely locked than before, deglutition was become impossible, and the pain under the ensiform cartilage was so extreme, as to force from the patient the most piercing cries. At this time the effects of the quicksilver ointment were apparent in the fœtor of the breath, and in a considerable salivation.

Had poor Gardner been a man of any rank, or indeed had he been surrounded by his family, it is most probable that we must now have abandoned him to his fate. But our proceedings being obstructed neither by the prejudices of ignorance, nor the weakness of affection, another, and a last effort for his life was resolved on. Having heard that the cold bath had been employed with success

success in tetanus in the West Indies, particularly by Dr. Wright of Jamaica, and Mr. Cochrane of Nevis, and this practice corresponding with certain speculations of my own, I had recourse to it on this occasion with some little confidence. With the consent of his officers, Gardner was carried to the public salt-water baths of this town, then of the temperature of 36° Fah$^{t}$. and thrown headlong into it. The good effects were instantaneous. As he rose from the first plunge, and lay struggling on the surface of the water, supported by two of his fellow-soldiers, we observed that he stretched out his left leg, which had been for some time retracted to the ham. But his head did not immediately recover the same freedom of motion, and therefore he was plunged down and raised to the surface successively for upwards of a minute longer, the muscles of the neck relaxing more and more after every plunge. When taken out, we felt some alarm: a general tremor was the only indication of life, the pulse and the respiration being nearly, if not entirely, suspended. Warm blankets however had been prepared, and general friction was diligently employed. The respiration and pulse became regular, the vital heat returned, the muscles continued free of constriction, and the patient fell into a quiet and profound sleep. In this he continued upwards of two hours, and when he awoke, to the astonishment of

of every one, he got up and walked across the room, complaining of nothing but hunger and debility. The convulsive hiccup indeed returned, but in a slight degree, and gave way to the use of the cold bath; which he continued daily a fortnight longer; and in less than a month we had the satisfaction of seeing our patient under arms, able for the service of his country.

That the opium, though it failed in effecting a cure, had considerable influence in mitigating the disease, and prolonging life, is, I think, apparent.

That the mercury had little effect, is clear from the second convulsions coming on soon after the salivation appeared.

The success of the cold bath in circumstances so apparently desperate was not lessened by bad effects of any kind. Though the patient was in a salivation when thrown into it, yet this was not stopped suddenly; it lessened indeed immediately, and soon disappeared, but without any of those bad consequences so well described by Sir John Sylvester and Dr. Dobson. Subsequent experience has taught me to attribute some part of the suddenness of the benefit obtained in this instance, to a circumstance that distressed me much at the moment.

moment. The very instant that we were about to immerge poor Gardner, he was seized with a general convulsion. We hesitated—but kept our purpose, and happily plunged him into the water with the convulsion upon him. I am also inclined to think, that our success is in part to be ascribed to the powerful, general, and sudden application of the remedy, and in this view of the subject, immersion is in certain circumstances perhaps preferable to affusion.

2. Soon after this I was sent for by a poor woman, who, in consequence of a difficult labour, and, as she imagined, of local injury in some part of the uterus, was seized with the spasmus cynicus, and locked jaw. She was immediately taken to the cold bath, and thrown into it in the same manner as the former patient, and with similar good effects. The spasms disappeared, and though they afterwards returned in a slight degree, they gave way entirely to a second immersion.

In the first of these cases I was assisted by Mr. Walker, surgeon's mate of the Staffordshire militia; and in the second case the effects of the cold bath were witnessed by him and my friend Mr. Park.

So

So far of the idopathic tetanus; my experience of the effects of the cold bath in the disease originating in wounds, is neither so satisfactory nor so complete.

3. The first case that occurred to me was that of a master of a vessel returning from sea in the year 1784. The injury had been received on the leg, but the wound had little inflammation on it, though the patient was so far advanced in the disease, as to go off in a general convulsion, before any remedy could be tried.

4. The second case seemed rather an instance of locked jaw, properly so called, than of tetanus. The injury having been received on the jaw itself, the affection was confined to the muscles of that part, and the disease seemed to be the trismus inflammatorius of Sauvages. It gradually yielded to bleeding, and the topical application of warm vapour, the cold bath producing no good effect.

5. In the third case, we contented ourselves with pouring cold water in successive buckets on the patient's head, partly because this was the most convenient method, and partly because it is that employed by Dr. Wright, whose paper on the subject, in the sixth volume of the Medical Observations and Inquiries, had by this time appeared.

While

While we were busy in this way, and flattering ourselves with some appearance of success, the unhappy patient suddenly threw himself on his back, and a quantity of water entered his mouth. The effects were highly distressing. The muscles of deglutition were unable to carry down or to expel the fluid, and such convulsions took place, as led us to expect instant death. He recovered, however, so far as to regain his former situation, but the effects of his sufferings on the by-standers prevented our continuing this practice, and he was carried off a few hours after.

6. A fourth instance of this disease I saw about two years and a half ago. There was no room for new practice, for the patient was too far advanced. He had been treated with opium and mercury as usual, and as usual died.

These cases afforded little inference either in favour of or against the use of the cold bath in tetanus arising from wounds, but they are stated shortly, that the account of my experience in this disease may be fair and complete.

It is worthy of remark, that the use of the cold bath in tetanus, is a practice of very ancient date. Dr. Wright, whose precision and candour are exemplary, mentions, that he received the first

first hint of it from Dr. Lind of Haslar. Hippocrates, however, was acquainted with it, as appears by Aphor. 21. lib. v. It is also noticed by Avicenna, lib. iii. cap. 7. and in the collection of Schenkius, p. 120, an account of the success of this practice in two cases of tetanus is quoted from *Jason*, cap. 21, *De morbis cerebri, ex Valeseo.* Hippocrates indeed, and Avicenna, restrict the use of this remedy to the young and corpulent, and to the summer season. They also confine it to cases in which the disease has not originated in local injury; for it appears by another of his aphorisms, that the father of physic thought convulsions supervening to a wound always fatal.* The experience of Dr. Wright encourages us to extend this remedy to tetanus originating in wounds, but if it should fail, we are still not to despair, as the following case will testify.

7. In July 1787, a labouring man was brought into our Infirmary with a wound in his hand. He was under Mr. Park's care, and the sore was nearly healed, when an alarming rigidity appeared in the motion of the jaw. Opium and mercury were immediately prescribed,



* Hippoc. Aph. lib. iv. sect. v. aph. 2.

with which were conjoined, first the cold, and afterwards the warm bath. Mr. Park, however, finding the disease to proceed with the most unfavourable omens, called a consultation, at which, in the absence of the rest of our colleagues, we agreed to make a trial of bark and wine, on the authority of Dr. Rush of Philadelphia, whose paper on this subject, in the second volume of the American Philosophical Transactions, had just appeared. We had, however, little or no hope, for the disease was far advanced, and its progress had been very rapid. At this time the jaw was not only rigidly contracted, but the spasms had extended to the neck and back, the pain under the ensiform cartilage was most acute, and twice or thrice in every hour he was seized with general convulsions, each of which lasted about half a minute.

The patient was, however, a man of a vigorous mind, and in his perfect senses; and his danger was not concealed from him. Death, he was told, must be the inevitable consequence, unless he swallowed wine in large quantity, but this we verily believed would save him. At first bark was infused in the wine, but he could not swallow the mixture, and therefore we trusted to wine alone. It was wonderful to see the exertions which this poor fellow made. If the liquid was offered to him

him at an improper time, the effort of deglutition brought on a general convulsion; nay a general convulsion was the consequence of advancing it at such a time towards his head. But watching the remission of the spasms, he was able to swallow a table spoonful or more at once, he himself giving the signal when the wine should be administered.* In this way, through the opening made by the lapping of the upper jaw over the under one, he drew up and swallowed a quart of port wine in the course of two hours; at the end of which time he thought himself refreshed, and was encouraged to proceed. At the end of twenty-four hours he had finished his third bottle, and at this time it was evident that the down-hill progress of the disease was checked. Though the pain under the ensiform cartilage (the most excruciating of all the symptoms of tetanus) was little abated, yet he felt himself more able to bear it; and the general convulsions were certainly less frequent and less severe. We continued our plan with patience and vigour, but for a long time life and



* This circumstance suggested to me the similarity between this case and hydrophobia. I noticed it to my respected friend Dr. Percival, who has mentioned it in his essay on that disease. *Vide* Percival's Essays, vol. ii. p. 366, 4th edition. I differ however from this experienced physician in his supposition that the two diseases are the same, or of the same origin.

death seemed to hang in equal scales. On the fifteenth day of this course he was affected with a slight nausea and vomiting, which soon went off, and thenceforward recovery seemed more apparent. It was not however till he had been forty-two days under this treatment that his safety could be ascertained, and during this time he swallowed a hundred and ten bottles of Port Wine.

In this case bark was administered along with the wine towards the latter period of the complaint; and during the whole of it, gruel or milk in the quantity of a quart a day, or upwards, was given by way of nourishment. The milk, however, he preferred, as agreeing better with the wine, and tending, as he conceived, to allay a burning sensation at his stomach, the effect of an acid ferment there.

Though the wine was given in such quantity, yet it never produced any symptom of ebriety—it soothed the irritation of his nerves, and comforted his mind, and, without increasing the frequency of his pulse, it augmented his strength.

Every night he took from sixty to a hundred and twenty drops of the tincture of opium, and, with the wine, this small dose seemed to have a more

more composing effect than thrice the quantity taken before he began it.

To allay the pain under the ensiform cartilage, cloths, dipped in æther, were applied externally with good effects. As I stood by him one day, pressing the dossils with my open hand to the scrobiculus cordis, he expressed himself relieved by the force which I applied. The same relief seemed to follow from a gentle and uniform pressure on the parts suffering from spasm. In consequence of this, rollers were applied round each thigh where spasmodic twitchings had been very distressing, and afterwards, at his own request, a general pressure of the same kind was applied to the back and abdomen. These bandages he would not suffer to be removed, but we poured æther on them occasionally, over the parts most affected by spasm, guarding in the usual way against the cold produced by too speedy an evaporation.

At length our patient recovered. He is now (1790) a watchman, and calls on me occasionally. He complains of a stiffness at times in the muscles of his back, and though naturally a strong and healthy looking man, his features retain the indelible impression of his disease. His eyes appear hollow, his face sharp and pale, his cheeks and lips

lips are skinny, and the masseter muscles hard and shrivelled.

I have lately had an account of a case of this kind which occurred in a sailor on the coast of Guinea, who recovered under the care of Mr. Robson, an ingenious surgeon in the African trade, from a state apparently desperate, by a very liberal use of bark and ardent spirits.

To conclude this part of my subject, I have known two instances of this disease in horses, both originating in wounds; the first of these was treated in the method first described. The cold bath was used repeatedly, and large doses of opium employed in the form of clysters; but the disease terminated fatally. The second of these occurred in a horse of Dr. Camplin's about a twelvemonth ago in his journey to Bath. The doctor informs me, that he gave him wine and opium, mixing sixty drops of laudanum with every pint of wine, and by this practice he had the pleasure of recovering him, though not till he had drunk as much wine as he was worth. Brandy or gin may perhaps be as successful in such cases.

It may be supposed, that these instances of success in the use of the cold bath in tetanus, would

would lead me to employ it in other convulsive disorders. This has accordingly been the case. Of the use of the cold bath in such disorders I can speak with some confidence, as my experience of it is now of several years' duration.

In the convulsions of children I have found the cold bath a most useful remedy, whether the disorder originated in worms or other causes. I have seldom known it to fail in stopping the paroxysms, at least for some time, and thereby giving an opportunity of employing the means fitted to remove the particular irritation. I must however observe, that in early infancy I have used it with caution, and generally by affusion, tempering the water when the weather was cold. I have in general made the application of cold in this way sudden and transient, have employed means to secure re-action, and have avoided the remedy entirely in all cases where the vital energy seemed much exhausted. With these precautions I have seen great success attend this remedy in a variety of instances. Minutes of several of these I have preserved, but I mean to give one only in detail.

John Slater, aged eight years, came under my care in the month of January, 1782. About two years before, while at play, he was seized

seized suddenly with a convulsion which continued for half an hour, and had returned ever since at short intervals. Various means had been employed for his relief, but without success; the fits were become more and more frequent, a hemiplegia had supervened, and the intellect was apparently lost. For twenty-four hours he had lain in a state of insensibility, motionless on the left side, and the muscles of the other side only agitated by convulsive tremors. We put him immediately into a tub of cold water, which instantly stopped the paroxysm, and threw him into a deep sleep, out of which he awaked after two hours with a shriek, and fell into convulsions as before. The cold bath was repeated, and afterwards continued daily, present relief being always obtained by it. The interval was employed in administering the tin powder, which was worked off with calomel, but no worms were observed to be discharged. After several days the convulsions returning, though with a considerable abatement, I became dissatisfied with the mode in which the bathing had been performed, the size of the tub employed never having admitted of sudden immersion. On this account we had the child conveyed to the public bath, into which he was thrown headlong, his father being stripped in the water to receive him. The temperature of the bath was 43° Fahrenheit. He was repeatedly plunged

plunged down and taken up for half a minute; was taken out of the water free of convulsion, fell immediately into a profound sleep, and awaked clear of complaint. In these respects this case resembles that of Gardner, first described. It resembles it also in this respect, that convulsion had taken place at the moment of immersion. The paralytic weakness of the side remained however for some time, but by the continued use of the bath, it was at length entirely removed; and the powers of the mind, which had been totally suspended, gradually returning, at the end of six months were perfectly restored.

He continued well upwards of twenty months, was healthy and vigorous in body, and in the acquirement of knowledge remarkably acute. But on the 30th of December, 1783, his mother having struck him and frightened him much, he was seized in the night during sleep with general convulsion, in which he continued several hours without intermission. Being again called to him, I employed the same remedy, but at the time of immersion the convulsion was not on him. Sleep and refreshment followed as before, but the paroxysm returned in the evening, though in a slighter degree. However, by perseverance in the daily use of the bath, and throwing him

twice

twice into the water with the *convulsions upon him*, in six days his health was restored.

I lament over the fate of this amiable boy. He continued from this time three years and a half in perfect health; grew strong and tall, and shewed great affection of temper and sensibility of mind. But being violently agitated with fear and grief, he fell again into convulsions in the month of July, 1787. I was sent for as usual, but was absent from town at a considerable distance. Having on former occasions launched him myself into the bath, his mother, a superstitious woman in a low walk of life, would not permit this remedy to be used till my return. He lay for thirty hours convulsed and senseless, and the first intelligence I had of his illness, was accompanied by the account of his death.

This case differs materially from the convulsions of early infancy. I give it therefore not as an instance of these, but as a specimen of a disease affecting children from four to twelve years of age, and which, though not very uncommon, as far as I can judge, has not yet found its place in any systematic work that I know of. The nosology of convulsive diseases is indeed very imperfect, and the terms we employ in describing them are by no means precise. Those who have

seen

seen tetanus and the disease last mentioned, will know, that what is called convulsion in both instances, is in the one case and the other a very different affection. The convulsion of tetanus is a sudden and violent contraction of all the voluntary muscles, throwing the frame into strong contortions, but lasting only a minute at most, and seldom so long. The patient, where the case terminates fatally, probably dies at last, from spasmodic affection extending to the heart, or sometimes perhaps, from the suspension of respiration. The convulsion in the other case, though it begins with violence, soon subsides, leaving the body in a state approaching to coma: and though the separate fibres of many of the muscles keep in constant vibration, and the turbulence of general convulsion returns occasionally, yet in the intervals the respiration is natural, and the pulse little disturbed; voluntary motion and consciousness, are, however, completely suspended.* If this state continues long, the violent commotions return more frequently, the breathing becomes laborious, the strength is at length exhausted, and the patient dies with the symptoms of apoplexy.

I conclude

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* This species of convulsion has been denominated *clonic*, in opposition to *tonic*, but with little propriety. All convulsions may be said to be clonic.

I conclude these details with a few general remarks.

1. It appears to me that the efficacy of the cold bath in convulsive disorders is much promoted by being employed during the presence of convulsion. How I came to be led into this opinion may be seen in the case of Gardner. Much experience is required to establish this as a general truth, and I give it as an opinion only. As madness is, I think, best combated in the height of the phrenzy, so I believe convulsive diseases are most capable of remedy in the agitation of convulsion. I seem to have a glimpse of a general principle in nature that connects these facts so apparently different; but the ground is not yet firm enough for the foundation of a theory.

2. It may however seem to strengthen the above remark, that in spasmodic diseases, which rise not to general convulsion, the cold bath seems to be of inferior efficacy. In Chorea Sancti Viti I have tried it frequently, but never found it of any service. This is one of the few diseases in which electricity is of decided advantage.

3. In the hysteric paroxysm, the cold bath, or indeed the plentiful affusion of cold water, is an

an infallible remedy. Those who suppose that the terror it occasions ought, in this case, to prevent our having recourse to it, are, in my opinion mistaken. Though the hysteric paroxysm be the offspring of passion, it is never occasioned, I will venture to assert, by the passion of fear. A sense of danger will always, I believe, prevent it; or indeed a powerful dread of any kind. I have known a tub of cold water kept in readiness, with the certainty of being plunged into it on the recurrence of the paroxysm, cure this disease, without the remedy being ever actually tried. I know the hysteric paroxysm often takes place when *danger is over*, but that is another case.

4. I have tried the cold bath in the epileptic paroxysm, but my experience of its effects is as yet (1790) too recent and too imperfect to be detailed. I am not so sanguine as to expect that experience will accumulate rapidly on this subject. Many there are who will think my practice too hazardous to be copied, even after this account of its success. This relates especially to the use of the cold bath in convulsive disorders in private practice; but in hospital practice, and in the practice of the fleet and army, the same objections do not present themselves; and in these departments of the profession, it may be expected that its effects will be ascertained.

Dr.

Dr. Girdlestone, in his account of the diseases among the troops in India, mentions, that hot Madeira wine is given with success in the tetanus which occurs in that climate; but he tells us, though not from his own experience, that the cold bath has been very unsuccessful. To what circumstance it may be owing, that a remedy which has been so efficacious in the West Indies, should have failed in the East, does not appear; though, if the circumstances under which it was employed were fully related, the difficulty might perhaps be explained. It may, however, be observed, that in the sultry climate of India, where the human frame is greatly relaxed, it will not be safe to use the cold bath with the same freedom as in more northern regions. And it will undoubtedly be more safe, in any future trials of this remedy in that country, to use the method of affusion employed by Dr. Wright, and so successful in his practice in a similar climate, instead of immersion, which it appears was employed in the cases mentioned by Dr. Girdlestone.

1798. Since the above was written, I have seen three different cases of tetanus arising from wounds, in which the affusion of cold water was employed alone, and in all of these the disease terminated fatally. I have seen a fourth case in which the affusion of cold water was conjoined with

with the liberal use of wine and opium, where the patient recovered. And I have seen two other cases in which the cure was trusted to wine and opium alone, in one of which the issue was fortunate, in the other unfortunate.

It is however proper to observe, that in the three first cases, the disease was far advanced, having resisted other powerful remedies before the patients fell under my care; and as the power of deglutition was nearly lost, the affusion of cold water was employed under desperate circumstances, as a *dernier ressort*. It was besides impossible to use this remedy to any extent, for the powers of life were too much exhausted to sustain the continued application, or the frequent repetition, of so violent a stimulus.—There is also in the advanced stage of tetanus, an objection to the use of a remedy that requires much change of posture. In such cases, every exertion of the will on the voluntary muscles has a tendency to induce a general convulsion.—From a consideration of all these circumstances, and from more extensive experience, I should now be disinclined to the use of the cold bath in any of its forms, in the symptomatic tetanus, unless in the earlier stages of the disease, when the vigour is less impaired, and the convulsive actions less firmly catenated.

The

The practice, first introduced by Dr. Rush, of giving wine in large quantities in tetanus, of which a successful case is detailed in this chapter, seems to me to be fully confirmed, as a most important part of the treatment of this very dangerous disease. The wine ought however to be combined with considerably larger doses of opium, according to my later experience, than in the case already referred to.

I have now employed the cold bath in other convulsive diseases for fourteen years, and on the whole, it has been attended with considerable success.—One conclusion, of rather a singular nature, was drawn from the cases formerly given, which my subsequent experience has uniformly corroborated:—" That the efficacy of the cold bath in convulsive disorders, is much promoted by its being employed during the presence of convulsion;" or, as I would now express it, *that the benefit derived from the cold bath in convulsive diseases, depends on its being used in the paroxysm of convulsion; that its efficacy consists in resolving or abating the paroxysm; and that when this effect is produced, the return of the paroxysm is greatly retarded, if not entirely prevented.* To this singular fact, for which I am unable to account, I invite the attention of physiologists: it may

may throw some light on the laws of association; a subject of such vast importance in physiology, and which hitherto has been so little understood. The following case, taken from the records of our Infirmary practice, is in many respects curious, and will serve to corroborate the position just laid down.

John Westmore, aged 22, was admitted into the Infirmary, on the 11th of October 1792. In consequence of a fright, he had been seized with fits two months before, which now recurred several times every day, of various duration, from two minutes to an hour. During these his consciousness was wholly abolished. These fits occurred without warning, and were peculiar in their appearance. At first the muscles of one side were strongly retracted, then those of the other, alternately; and then the muscles of both sides acting together, the whole trunk of the body was drawn upwards to the head; this action, resembling very exactly that of the victim of the law, suspended from the gallows in the agonies of death. His faculties of mind did not seem as yet impaired, nor the animal functions much disturbed. No medicines were ordered for this patient, but a bucket-full of cold water was directed to be thrown over him, the instant of the accession of the fit. Some circumstances prevented

this being done for a few days, during which he went into the cold bath daily when the fits were off him, and with seeming advantage; the number of paroxysms being reduced from eight or ten in the twenty-four hours to two or three. At length he was thrown into the cold bath on the instant of the accession of one of his fits, which was speedily terminated, and from this time forth he had no return for fourteen days, when he was discharged as cured. Westmore continued free of complaint till the beginning of February following, when his fits returned. He was re-admitted on the 24th of that month, and at that time, his fits recurred six or seven times in the day. He was again directed to use the cold bath daily—and to have a bucket-full of water thrown over him on the accession of every fit. By this practice he speedily recovered, and since that time the disease has never returned. He is now, (*March* 1798) in perfect health.

(1803) Westmore continued well till the month of January last, when he was again seized with a convulsion, and applied to me as formerly. I ordered the cold bath to be used on the recurrence of the fit, and in the interval directed him to take a grain and a half of the nitrate of silver daily. The convulsion has never returned, but about three weeks after he began this medicine, he

he parted with two yards of the tape-worm. He seems again (*June* 1803), in perfect health. The nitrate of silver seems also to have succeeded with me in two cases of epilepsy.

I have used the cold bath during the paroxysm of epilepsy in various instances, but in general without the patient being roused to consciousness or sensation, and without advantage. In one case of this disease, which occurred in the hospital practice, the paroxysm returned periodically every afternoon. In this instance a cure was effected by applying a cataplasm, formed chiefly of tobacco, to the scrobiculus cordis, about half an hour before the expected return, by which a powerful impression on the system was produced, and the paroxysm of epilepsy prevented. This practice repeated several days at the expected periods, probably destroyed the diseased association, for the cure was permanent.—In this mode of application, the peculiar effects of tobacco on the nervous system are speedily produced, and it is perhaps the safest way of exciting them—because the application can be discontinued at pleasure; an advantage of great importance in the use of so deleterious a medicine, and which cannot be obtained when it is thrown into the stomach or rectum. I was induced to use tobacco on this occasion, from having observed it to succeed in the cure of obsti-

nate intermittents, when applied in the same manner, previous to the expected accession of the paroxysm. In two recent cases I have employed this remedy in a different manner. Each of the patients laboured under general convulsion, and the paroxysm had returned so frequently, as to produce continued coma, and in the by-standers an absolute despair of recovery. I ordered a decoction of half a drachm of tobacco in four ounces of water, to be thrown up as an enema. This powerful agent penetrated the system to its very centre, roused the sensibility which the affusion of cold water, and other external impressions, could not awake ; excited sickness, vomiting, and profuse perspiration, and interrupted the convulsive actions, which have never since returned. In each of these cases the recovery was altogether unexpected. The use of tobacco smoke thrown up the rectum, in obstinate constrictions of the alimentary canal, has been long established. I believe the effects may be obtained with more ease, and greater certainty, by employing the decoction. The reasonings used in explaining the operation of the affusion of cold water, are perfectly applicable in accounting for the effects of tobacco ; and there is reason to hope, that convulsions stopped in their paroxysm, by the one or the other remedy, are not in many cases liable to return.

In

In spasmodic diseases which rise into general convulsions, I have seen these convulsions rendered less frequent, and for a time seemingly diminished in violence, by medicines used during the intervals; but seldom effectually cured, except by means used during the act of convulsion, at the instant of its accession, or at the period of its expected return. Of the medicines employed during the intervals of epilepsy, the oxyd of zinc seems to me to have some efficacy; but the digitalis purpurea still more.—In consequence of its powerful influence, I have used this last medicine in many cases of epilepsy, both in private and hospital practice, of twelve of which I have preserved particular registers. *In every one of these*, it diminished the number of paroxysms, and the intervals were so much lengthened, that in several instances we entertained hopes of a cure. It did not however finally succeed in any one of them; and when, as the constitution became accustomed to its influence, the fits began to return, and we increased the dose of the medicine, its deleterious qualities, so well described by Dr. Withering, presented a danger greater than that of the disease, and obliged us to abandon it. On one occasion, we had an opportunity of observing, that pushing this medicine to the utmost extremity did not effect a cure. A female patient took it so rashly as to produce the utmost danger of her life. Her pulse

pulse sunk to thirty-two in the minute, her heat at the axilla to 89°;—her sight failed, her respiration became laborious, and incessant vomitings threatened immediate dissolution. She was however recovered by the warm bath and opiate clysters, but as her strength was restored, the epileptic paroxysms returned.

If it were my object to give a general view of the effects of the cold bath, or the affusion of cold water, in spasmodic affections, I might quote largely from the systematic writers of the last century, and I might detail various cases which have been furnished me by respectable living practitioners.—My friend Dr. Ford has mentioned to me the case of Mr. C. of Bristol, who was instantly relieved of an obstinate stricture of the neck of the bladder, of thirty hours' duration, (during all which time not a drop of water had passed) by placing his feet on a marble slab, and dashing cold water over the thighs and legs. The effect was instantaneous; the urine burst from him in a full stream, and the stricture was permanently removed. The common remedies, particularly opium and bleeding, and each of these very largely, had previously been used in vain. I record this case with the more pleasure, as it will recall similar cases to the learned reader's memory, and indicate a numerous class of diseases of the same

same nature, in which this powerful remedy may be employed.

Cold water cannot be used as a drink during the paroxysm of convulsions, and of course we cannot shew the analogy between its external and internal use in these, as in other diseases. That its effects taken internally, are most salutary in a numerous class of chronic diseases, is however well known, though perhaps not acknowledged to the full extent of the truth. A considerable part of the virtue of mineral waters is, doubtless, to be attributed either to the diluting quality of the pure element itself, or to the invigorating effect of cold on the stomach, and through it on the system at large. The subject is however sufficiently extensive, as well as important, to require and to deserve a distinct examination. In hypochondriacal, hysterical, and dyspeptic affections, cold water taken internally has produced the most salutary effects. Hoffman praises it in head-ach, whether arising from indigestion, or some primary affection of the nerves of the head. The following case, extracted from his first volume, *p.* 475, will shew the use of cold drink in certain convulsive affections.

" *Puer hebræus duodecim annorum, a terrore & frigore, incidit in ingentes præcordiorum anxietates*

*tates, cum tussi, virium lapsu, extremorum tremore, intercurrente horripilatione & interno æstu. Tertiâ die post manus & pedes convulsivis motibus & subsultibus agitati; per horæ dimidium insistentibus. Inde magis magisque increverunt convulsivi hi motus, ut dorsum & collum aspectu horrendis incurvationibus quassarentur, idque aliquoties quotidie. Sub paroxysmo pedes frigebant, respiratio erat crebra, pulsus celer & durus, sensus tamen constabant. Post paroxysmum languebat corpus universum, appetitus vero cum somno erat integer. Adhibiti in consilio medici multi; exterorum etiam consilia conquisita. Plurimi verminosam progeniem subesse causam rati, aliqui per terrorem invectum esse malum putarunt. Sed data, tam ex illorum scientiâ anthelmentica, quam ex horum præscripto antepeleptica, adhibitis simul balneis, fuerunt prorsus frustranea. Malum potius perstitit idem, quin subinde factum acerbius. Tandem miser aquam fontanam frigidam, ad unam vel dimidiam mensuram, quotidie per vices bibere jussus est. Quo intra 14 dies motus isti convulsivi paullatim compositi, nec in hoc usque tempus iterum infestarunt.*" This simple and successful practice deserves to be imitated.

Since the last edition of this volume, two cases of tetanus have occurred to me in private practice,

practice, both arising from wounds, both extremely formidable in their symptoms, and both terminating happily. I think it my duty to give some account of them here, as exhibiting the combined effects of the different remedies employed in the former cases, and presenting my last and most successful treatment of this terrible disease.

Mr. A******, forty-four years of age, and of a vigorous constitution, in returning home in the evening of the 26th of January, 1799, fell into a cellar and received an extensive wound in his leg. He was attended by my friend Mr. Minshull, now surgeon to the Liverpool Infirmary, who applied the proper dressings, and gave him some laxative medicine. The wound not healing kindly, bark was ordered on the 5th of February, which he continued till the 10th, on the evening of which day, a stiffness in the jaw first appeared. On the 11th he began to take opium, and on the morning of the 12th, when I first saw him, he had taken six grains of this medicine, but his disease was making rapid progress. We therefore increased the quantity of opium. He was directed to take one grain every two hours, and three grains at once at bed-time; and he was desired to take at least one bottle of wine

wine in twenty-four hours, with as much gruel and jelly as his stomach would bear.

Though Mr. A. now swallowed with great difficulty, and at intervals only, he was able to conform in a great measure to these directions. He got down twelve grains of opium, and rather more than a bottle of wine, daily, with a sufficient quantity of jelly and milk (which last he preferred to gruel) for the ordinary demands of nutrition. The progress of the symptoms was somewhat retarded, but not suspended; pain and stricture came on under the ensiform cartilage, and dossils, dipped in æther and laudanum, were kept constantly applied with a gentle pressure to the scrobiculus cordis. The same application was occasionally made to the muscles of the thighs. On the sixteenth, the disease was however, evidently increased.—The muscles of the neck and back were now powerfully constricted, and the pain under the sternum, produced from time to time those sudden and violent contractions, which convulse the whole system, and render the breathing at the moment difficult and irregular. There seemed nothing to hope unless some more powerful means could be adopted. In addition to the remedies already employed, we determined therefore to have recourse to the affusion of cold water on the surface, but having been somewhat discouraged

discouraged by the cases mentioned, *p.* 143, I desired the water to be raised to 75° of Fah^t., which is nearly the temperature of the springs, and of the sea in the West Indies, where Dr. Wright had employed this remedy with so much success. Mr. A. found considerable and immediate relief from the affusion, which, at his own request, was constantly repeated whenever the symptoms were most severe, and always with sensible and instant benefit. From this time the symptoms became stationary: in a few days there was an abatement of their violence, and under a continuation of this treatment, he finally recovered. The case was however a considerable time doubtful; it was not till after the expiration of twenty days, that we could consider the recovery as certain. He took during this time, on a medium, twelve grains of opium, and nearly three pints of wine in the twenty-four hours, and had the cold affusion between three and four times daily. The costive effects of opium were obviated by calomel, by which, at one time, his gums were slightly affected. From the beginning of April, the opium was reduced to one grain at bed time, and the bathing was entirely omitted. He took in all, two hundred and seventy grains of opium, and used eighteen ounces of the tincture of opium in embrocation, with twice the quantity of æther. The wound in the leg, which was deep, continued

nued open till the month of June, when it healed kindly. Mr. A. is now living and in perfect health.

The other case is still more remarkable.

In the beginning of April (1803) a splinter ran in under the nail of the middle finger of Mr. Payne, of the house of Wood and Payne, merchants, and pierced as far as the first joint: a part of the splinter was removed, but a part remained in the wound, and the finger continued painful. He however thought slightly of the circumstance, and did not intermit his usual occupations. On the 11th of the month he had a slight stiffness of the jaw, which increasing, he called on Mr. Minshull, by whom my assistance was requested. I saw him first on the 13th, and the symptoms were then increasing hourly. Mr. Minshull had removed the remaining part of the splinter, and taken off the nail entirely. A poultice had been applied to the finger, which was now easy. The local irritation speedily ceased, and the wound healed in a few days. But the affection communicated to the system, was not in the slightest degree alleviated by the removal of the irritation which originally produced it, a fact which is consonant to invariable experience in this disease, and which points out the impropriety of having

having recourse to amputation for the purpose of removing this irritation, as has been too often practised.

The symptoms of Mr. P. had the most formidable appearance, and encouraged by our success in the preceding case, we adopted nearly the same treatment, but with greater boldness. Not being able to swallow opium in the form of pills, he took a mixture, containing about one hundred and sixty drops of the tincture (in doses of about twenty drops at a time) every twenty-four hours; an embrocation of the tincture, with twice the quantity of æther, was applied externally to the scrobiculus cordis, and to such other parts as were most painfully constricted, and on such parts we also rubbed in an opiate ointment, though not in any great quantity; for not finding immediate relief from it, he preferred the embrocation. He was directed to drink wine freely, and to swallow as much nourishment as he could bear. And the cold affusion was directed to be tried, and to be repeated, according as he might find benefit from it, and the state of his disease might permit the exertion requisite in using it. Mr. P. was made fully sensible of his situation, and of the exertions required to give him a chance of recovery. Being in the vigour of life, and of great bodily strength and resolution, the exertions he made were very uncommon.

tremity from the 16th to the 20th of April. During these four days he used the cold affusion twenty-three times, and the water employed at each time was by his own desire doubled in quantity. The efforts required to move him out of bed were most painful and difficult. Happily the muscles of his arms and shoulders were less affected than those of the rest of his body, and he was able to make some exertions with them. But it was often fifteen or twenty minutes before he could turn his legs over the side of the bed, into the tub in which he stood while the water was poured over him; and the process of his rising was always interrupted by one or more convulsions. In one of these he seized hold of the post of the bed with his hands, and stood upright, so rigidly constricted, that he could not change his position for two hours, the sweat all the time pouring in torrents over him.

Notwithstanding the violence of the disease, and the vast quantity of wine and opium which he swallowed, Mr. P.'s heat was never greater than the natural standard. The superfluous heat which these medicines ought to have produced, was no doubt carried off by the profuse and constant perspiration. Neither did I observe his heat, except in a few instances, below the standard of health, and there seems little doubt, that the wine

and

and opium enabled him to sustain the cold affusion under the profuse perspirations which they excited. On two or three occasions, when he was stripped for the bath, and delayed by spasmodic affections from using it, he was seized with cold chills and general rigors. In these instances he was replaced in bed, and restored to heat by two persons stripping and lying down on each side of him. Under the treatment I have mentioned, and the profuse perspiration which attended it, the surface of Mr. P.'s body became extremely sensible to impressions, especially of cold, or of the motion of the air; his situation in this respect resembling what occurs in hydrophobia, and seems to be essential to that disease. The sensibility of his mind suffered a corresponding increase (as is I believe usual in such cases), and his spirits became almost hysterical. He is now (June 24th, 1803), in all respects in his usual health, excepting that some bodily weakness remains, and that there is some depression on his spirits, perhaps arising from the sudden change in his habits as to the use of strong liquors, for which he has taken a decided aversion.—It ought to have been mentioned, that both in this case and in the preceding one, great relief was obtained from the use of laxative glysters.

These cases are, I hope, decisive, in regard to the general principles on which tetanus should be

 treated.

treated. The methods which were employed, will, I trust, be one day simplified and improved. If a new case should occur, it is my intention to try opiate frictions more freely. In this way the effects of the medicine may be introduced into the system, and perhaps much of the pain, difficulty, and hazard of deglutition spared.

It is deeply to be lamented, that this disease should ever have been considered as of an inflammatory nature, and that there are even now, physicians who treat it by venesection. It is in my mind decisive against this supposition, that though the general system is so powerfully affected, the animal heat is not increased, which it uniformly is, so far as my observations extend, in all cases where there is an inflammatory affection of the system, whether originating or terminating in local phlegmonic inflammation.

The same consideration is decisive with me against the supposed inflammatory nature of hydrophobia, a notion which some respectable physicians have lately revived. Of this most singular and affecting disease, I have seen five cases, in none of which was there any increase of animal heat. All these terminated fatally. Hydrophobia has some resemblance to tetanus, but is however in my judgment, obviously and essentially different.

ent. In the case of a maid servant of my friend Mr. Roscoe, (so well known in the republic of letters) it was ascertained, that this disease proceeded from the saliva of a rabid dog, applied to the inside of the lip without any wound having been produced—a circumstance which is in itself decisive against the identity of hydrophobia and tetanus. I have seen two dissections of persons dying of hydrophobia. In both of these there were some slight appearances of congestion in the blood-vessels of the œsophagus; and on the surface of the stomach, but such only as the convulsive motions might easily explain.

# CHAP. XIV.

## *Use of the Cold Bath in a Case of Insanity.— General Reflections.*

I Have mentioned before, the connexion between convulsive diseases and insanity, and conjectured that the same principles are in many cases applicable to the treatment of both.* The following case, taken from the register of our Asylum, may serve as a new proof of this connexion.

*June* 2, 1796. L. I. aged 32, a man of very irregular habits of life, was admitted into the Asylum, in a state of furious insanity. His disease was supposed to have been brought on by excessive

* This connexion has been explained, and the subject very ingeniously illustrated by Dr. Darwin, (*Zoonomia, vol.* ii. *c.* 3.) who has arranged the whole of these diseases under one class, and if the truth of his first principles were established, might be said to have thrown a new light on one of the most curious and obscure parts of pathology.

excessive drinking. As cases of insanity, including the whole method of cure, are seldom recorded, I will give this in detail.

Very powerful methods of coercion were required in the first instance, and the bowels were afterwards cleared by a saline purgative. At night he took eighty drops of laudanum, which produced imperfect sleep, and in the morning following he was more wild than ever.

Having in some cases of insanity, found very extraordinary effects from such doses of opium as induced profound sleep, I directed that five grains of this medicine should be given every four hours, till sleep succeeded. Next day (4th *June*) it was reported to me, that he had taken twenty-seven grains of opium in the twenty-four hours. From the first five grains there was no apparent effect; after the second, he slept an hour and a half; after the third, three hours. Though the medicine had been since twice repeated, in doses of six grains each, he had no return of sleep—He appeared wild, agitated, and feeble, rather in a state of alarm than of fury. His pulse 100, and soft, with gentle and general diaphoresis.

It seemed not prudent to push the medicine farther; but having often, in cases of this nature, observed

served the soporific effects of opium to come on many hours after the medicine had been discontinued, it was directed that he should have milk as his food, and that medicines of every kind should be omitted for twenty-four hours.

The report of the fifth is as follows. "He remained in a perpetual state of alarm till eight o'clock last night; after that hour he slept, but not soundly. He has taken nine pints of milk in all, having been very thirsty. It agreed with him till ten this forenoon, when he vomited considerably. Since that time he has been more tranquil and feeble." In this state, I continued the directions to omit all medicines, ordering him a pint of wine, with two pints of gruel daily, and such solid food as his stomach could receive and digest. This plan was followed till the 17th, when his strength was considerably restored, but his insanity as violent as ever. It was now directed that he should go daily into the bath, heated to 95°, and that he should take half an ounce of the infusion of the digitalis thrice a day, as recommended by my friend and kinsman, Dr. Currie of Chester. (*Mem. of the Medical Society of London, vol.* iv.)

He continued on this plan with little variation till the 29th; the report of that day is as follows: "Till

" Till the 25th the digitalis produced no obvious effect. On that day sickness came on, and his pulse was hardly perceptible. The digitalis was therefore omitted. He has been regularly in the bath, and while in it was once seized with *deliquium animi*, but the mental derangement did not afterwards abate. His pulse is now about 94, feeble and irregular.—He is a good deal emaciated, but more fierce than ever; he talks incessantly, with the utmost incoherence, and the association of his ideas seems wholly perverted."

In this state I ordered the digitalis and the tepid bath to be discontinued, as well as all other medicine. I directed a nutritious diet, but without wine or opium, and placed him in a situation to be as little as possible exposed to external impressions. For three days there was little change in his situation—his appetite improved, and he sweated much. At the end of this period, he had suddenly a lucid interval of several hours' duration; but before I could witness it, he was again become furious.

At this time (the 16th July) I directed that he should take three drachms of peruvian bark, half a drachm of vitriolated iron, and the same quantity of aromatic powder, in the form of an electuary, daily; and that twice every day, he should be

be subjected to the tepid affusion for a minute or two at a time, the water being poured gently over him of the heat of 88°.

The following is the report of the 16th *July*: "The tepid affusion was continued till the 14th, but without any tranquillizing effect; the other medicines have been given regularly. On the 9th he became calm and rational, and slept the following night for eleven hours profoundly; and started suddenly from this sleep, as wild and furious as ever. This state of mind continued twenty-four hours, when he again became tranquil, and continued so till he went to bed. He slept a few hours quietly, but awoke as before, under extreme agitation, and continued fierce and raving for twelve hours and upwards. Since this time, he has had a succession of paroxysms of insanity, alternating with periods of tranquillity of mind, of nearly the same duration. In general he goes to bed tranquil, and sleeps quietly till about three in the morning, when he awakes under the horror of some frightful dream; and this introduces his paroxysm of furious insanity, which continues from ten to fourteen hours."

In this situation I ordered an emetic to be administered to him late in the evening, in hopes that it might prevent the return of the paroxysm.

Being

Being disappointed, I had again recourse to an opiate at bed-time; and this also proving inefficacious, I directed that he should be awoke before the expected accession, and kept awake beyond its period of attack. By these means we disturbed the regularity of the alternations, but without any advantage; on the contrary, his lucid intervals became shorter in duration, and less complete.

Perplexed with these extremes, and having in mind the success of the cold bath in convulsive diseases, I ordered it to be tried on the present occasion. The insanity returning with great violence on the 21st, he was thrown headlong into the cold bath—He came out calm, and nearly rational, and this interval of reason continued for twenty-four hours. The same practice was directed to be repeated, as often as the state of insanity recurred.

The following is the report of the 30th: "The direction has been followed, and on the morning of the 23d, he was again thrown into the cold bath in the height of his fury, as before. As he came out, he was thrown in again, and this was repeated five different times, till he could not leave the bath without assistance. He became perfectly calm and rational in the bath, and has remained so ever since."

This

This patient continued with us for some time afterwards, bathing every other day, and taking the oxyd of zinc in small quantities. He never relapsed, and was discharged some time afterwards, in perfect health of body and mind.*

In

* It has been contended by Dr. Darwin, that as convulsive diseases arise from inordinate action in the muscles, so maniacal diseases arise from the same inordinate action in the organs of sense:—and as the muscles, and the organs of sense, are the proper instruments of the will, the diseased actions of the one and the other are by him arranged as one class, under the title of *Diseases of Volition*. But in the system of Dr. Darwin, the actions of the organs of sense constitute our ideas; a deranged action of these organs is therefore no other than what is usually denominated a derangement of mind; in other words insanity. As therefore convulsion and madness, may be considered as the same disease differently situated; and as the seat of the disease may be changed from various causes, and extended through various sympathies and catenations—if the proposition of Dr. Darwin, that our ideas are motions in the organs of sense, were established, we should have an explanation of that connexion between convulsive affections and insanity, and particularly of their alternations with each other, which have been observed in every period of medicine.—That several of the modes of treatment applicable to the one, are also applicable to the other, has long been known. The arnica montana, the oxyd of zinc, the digitalis purpurea, and opium, are medicines used equally in convulsions and in insanity. The case which has just been related extends this analogy, and is the more striking in this point of view, because

In order however to the success of the cold bath, either in mania or convulsion, it will be readily

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because the success of the cold bath was almost instantaneous, and occurred in the paroxysm of insanity, in the same manner as has been before observed of its operation in convulsion.

It is supposed by Dr. Darwin, that the motions excited by volition and by sensation, proceed in different directions; the first passing from the central parts of the system towards the superficies, the second from the superficies towards the central parts. Hence he concludes, that they are opposed to, and mutually destructive of each other. To increase the force of voluntary actions must therefore be to lessen positively, as well as relatively, the actions that arise from sensation, and the converse of this proposition is equally true. Whatever faith we may give to the hypothesis of Dr. Darwin, his inference seems agreeable to a great variety of phenomena. That the higher degrees of volition and sensation are incompatible with each other, is a truth of great importance in every part of the science of life, whether moral or physical. It was the doctrine of another great physiologist, whose praise will now be heard with approbation, since he is beyond the reach of praise, " that no two actions can take place in the same constitution, nor in the same part, at the same time;" (*John Hunter, Treatise on the Blood—Introd. p.* 3) and though he applied this doctrine chiefly to show the incompatibility of different diseased actions with each other, yet it seems applicable also to explain the operation of a great part of the more powerful remedies. The position of Dr. Darwin resolves itself into this more general principle of Mr. Hunter, and the operation of the cold bath in

readily perceived, that it is necessary that no considerable læsion of structure shall have taken place, and that the morbid actions be not so strong as to produce insensibility to impression, and shall not have been so often repeated as to form indissoluble associations. It seems also necessary that the diseases in question shall be such as are susceptible of general paroxysms; a circumstance not so easily explained. Finally, it is necessary that the digestion shall not have been much impaired, or the vigour of the circulation much debilitated, lest the action of cold be too strong for the living powers. Under these restrictions, the cold bath, according to my experience, may often be applied with advantage, and always with safety, in convulsive diseases, and in insanity; for though in these diseases, even in their more violent forms, the temperature of the body

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in the paroxysm of madness, or of convulsion, receives an easy explanation under either doctrine. We may say with Dr. Darwin, that the powerful stimulus given to sensation destroys the previous diseased volition, or in the more general and more simple language of Mr. Hunter, that the sudden application of cold water to the surface, produces an action of the system with which the previous diseased action is incompatible. Or, what would perhaps be more prudent than either, we may defer all attempts at explanation till observation and legitimate induction have enlarged our knowledge of physiology.

body is little, if at all, increased, yet the actual heat is retained with great tenacity, as I have found by many observations.

The resistance which the fierce maniac opposes to cold, he opposes also to contagion, and to other noxious powers, as has often been observed with surprise and admiration. This resistance seems to belong to every species of increased voluntary exertion, where the functions of digestion and nutrition are not impaired. The illustration of this position would not be difficult, and the conclusions to be deduced from it are in a high degree interesting and important; but a discussion of the subject would lead us too deep into the general doctrines of life.*

CHAP.

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* (1798) Dr. Marcard, of Pyrmont, in his work on the effects of warm and cold baths, entitled, *Ueber die Natur, und Gebrauch der Bader*, lately published, has quoted what I have said on the effects of the cold bath in convulsive diseases, and it appears, that he does not assent to my conclusions. Dr. Marcard supposes indeed, that the cold bath may be useful in convulsive diseases, where there is no local affection stimulating the system, but he contends, that it *must* be hurtful in cases where convulsions originate in stimuli, as worms, or crudities in the alimentary canal, teething, &c. It does not appear to me that Dr. Marcard has understood my paper perfectly, and I do not feel myself disposed to yield my experience to his speculations. The doctrine which Dr. Marcard

# CHAP. XV.

*An Account of the remarkable Effects of a Shipwreck on the Mariners; with Experiments and Observations on the Influence of immersion in fresh and salt Water, hot and cold, on the Powers of the living Body.*

THOUGH I have already declared, that it is not my intention to go at present into the general effects of temperature on life, yet, as I wish to present,

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Marcard seems to maintain, that where one stimulus is already acting, the application of another must be injurious, is contrary to experience, and as I apprehend, to the laws of the animal œconomy. The work of Dr. Marcard is a valuable one, and deserves to be translated; but it seems to be too full of those general and sweeping observations, which, though nearly banished from other sciences, continue in the writings of physicians, and serve at once as a proof and a cause of the imperfection of our art. I fear this volume will not be found free of them.

present, under one view, what may be subservient to this purpose at some future period, I shall here insert the substance of a paper read before the Royal Society, and inserted in their transactions for 1792.

On the 13th of December, 1790, an American ship was cast away on a sand-bank that lies in the opening of the River Mersey into the Irish Channel. The crew got on a part of the wreck, where they passed the night; and a signal which they made being discovered next day from Hillberry Island, a boat went off, though at a great risk, and took up the survivors. The unfortunate men had remained twenty-three hours on the wreck; and of fourteen, the original number, eleven were still alive, all of whom in the end recovered. Of the three that perished, one was the master of the vessel; another a passenger, who had been a master, but had lost or sold his ship in America; the third was the cook. The bodies of these unfortunate persons were also brought off by the men from Hilberry Island, and were afterwards interred in St. Nicholas's church yard, amidst a great crowd of spectators. The cook, who was a weakly man, died a few hours before the boat reached the wreck, but the two masters had been long dead, and this united the sympathy for their loss, with a curiosity to inquire

quire into its circumstances and causes. When the following particulars came to be known, this curiosity was increased. Both the masters were strong and healthy men, and one of them a native of Scotland, in the flower of his life, early inured to cold and hardships, and very vigorous both in body and mind. On the other hand, several of the survivors were by no means strong men, most of them had been inured to the warm climate of Carolina, and what was singular enough, the person among the whole who seemed to have suffered least, was a negro.

What is extraordinary is seldom long unaccounted for in one way or other, and the death of the two masters was said to have been owing to their having taken possession of a keg which had contained cherry brandy, and which still contained the cherries;—these, it was reported, they had kept to themselves, and eaten in large quantities after the shipwreck; and this having produced intoxication, was supposed to have hastened their death. Some experienced seamen were satisfied with this account, which indeed seemed very rational; for though spirituous liquors may fortify the body against the effects of heat, combined with moisture, and may perhaps support it for a short time under great fatigue, they are, I believe uniformly hurtful, when taken under

der severe and continued cold. Pleased to see a doctrine becoming popular, which has been so ably supported by Dr. Aikin,* and others, I believed it might receive a striking confirmation from this catastrophe, into the particulars of which I determined to examine accurately. I therefore obtained access to the survivors of the crew, and from them, but more especially from Mr. Amyat, the mate, I received the information which I required.

In repeated conversations with this intelligent young man, I learnt, that Capt. Scott, the master of the vessel, died in about four hours after the ship struck; and that Capt. Davison, the passenger, died in about seven; but that the story of their having eaten cherries infused in brandy, was entirely without foundation: of this he was certain, for he saw the keg, which contained the cherries, staved, while Capt. Davison was endeavouring to fill it with water to make grog for the crew; the cherries fell on the wreck, and were immediately washed into the sea. Mr. Amyat expressed his surprise at the early death of the two masters, but could not assign any cause



* See Transactions of the Philosophical and Literary Society of Manchester, v. 1.

for it. He said there was no liquor of any kind saved, nor any sort of food; that the whole crew were on an equality in all points, except that some were deeper in the water than others, but that the two masters had the advantage in this respect, for they sat on the only part of the wreck that was out of the sea, whereas the poor negro, who escaped almost unhurt, was perhaps deepest in the sea of any. He explained this in the following manner. When the ship struck, they cut away her masts to prevent her from oversetting, and after this she drifted over the sand-bank, into what is called a "swash," on the other side. Here she floated, and they let go their best bower anchor; but it dragged, and the vessel struck again in a few minutes on another bank. In this situation she lay some time, beating against the sand, and the sea breaking over her. In a little while Mr. Amyat saw the tar barrels, which formed her cargo, floating towards the land, and soon after the bottom parted entirely, and was carried in the same direction. Happily for the men, the part of the wreck on which they were lashed, was held by the anchor, and floated in the water, a small portion of the after part of the quarter-deck being above the surface. On this sat the two masters, generally out of the sea, but frequently overwhelmed by the surge, and at other times exposed to heavy showers of sleet and snow,

snow, and to a high and piercing wind. The temperature of the air, as nearly as can be guessed, was from 30° to 33° of Fah[t]. and that of the sea, from trials in similar circumstances, from 38° to 40°.* Immediately before the two masters was Mr. Amyat himself. As he was sitting, and the deck sloped pretty rapidly, he was generally up to the middle in the water. The situation of the rest may be supposed; some of them were up to the shoulders. They were not at any time able to change their position, but kept their legs in pretty constant motion, to counteract the cold, their arms being employed in holding by the wreck.

The master of the ship, Captain Scott, a native of North-Carolina, and about forty years of age, died first. As they were in the dark, Mr. Amyat could not see his countenance; but he was first alarmed by hearing him talk incoherently, like one in the delirium of fever. By degrees his voice dwindled into a mutter, and his hearing seemed to fail. At length he raised himself up in a sort of convulsive motion, in which he continued a few seconds, and then fell back dead on



* (1803.) From some recent experiments I have reason to believe, that the temperature of the sea was not higher than that of the air.

the deck. This happened about eight in the evening; four hours after the ship went aground. Soon after this, Captain Davison, who was about twenty-eight, began to talk incoherently, in the same manner as the other. He struggled longer, but died in the same way, at about eleven at night. The cook died in the forenoon of the succeeding day. He was a low-spirited man, and desponded from the beginning. All the rest held out, as has been already mentioned, though sorely pinched with cold and hunger, till they were taken up about three in the afternoon. Mr. Amyat said that his hands and feet were swelled and numb, though not absolutely senseless; he felt a tightness at the pit of his stomach, and his mouth and lips were parched; but what distressed him most, were cramps in the muscles of his sides and hips, which were drawn into knots. Though immersed in the sea, they were all of them very thirsty; and though exposed to such severe cold, Mr. Amyat himself was not drowsy, nor were any of the men drowsy, nor did sleep precede death in those that perished. These facts are curious.

Reflecting on the particulars of this melancholy story, there seemed no doubt, that the death of the two masters was to be imputed to their peculiar position on the wreck. Exposed to heavy showers of sleet and snow, they might suffer from being wet with

with fresh, rather than salt water; they might also suffer from being exposed to the cold of the atmosphere, probably seven or eight degrees greater than that of the sea.* The chilling effects of evaporation might operate against them, promoted as these must have been by the high wind; or they might receive injury from their frequent immersions in the sea, producing an *alternation* in the media surrounding them. This last supposition did not, indeed, strike me at this time; the others dwelt on my mind.

Of the powers attending animation, that which seems fundamental, is the capacity of the living body of preserving the same heat in various degrees of temperature of the same medium, and, indeed, in media of very different density and pressure. If a definition of life were required, it is on this faculty that it might best be founded. It is known that some fluids, applied to the skin, vary in their effects according to their impregnation. In the same degree of temperature, pure water on the surface of the body is much more hurtful

* (1803.) This is I now believe a wrong supposition. I do not believe there was any difference. The water spreads over a vast expansion of sand, and as the cold had been some time great, it had probably brought both elements to the same temperature.

hurtful than water in which salt is dissolved. Seafaring men are universally acquainted with this, and a striking proof of the truth, as well as of the importance of the observation, may be found in the narrative of Lieut. Bligh. Probably the saline impregnation, may stimulate the vessels of the skin in some way that counteracts the sedative or debilitating action of the cold. At any rate, it seemed not unlikely, that some light might be thrown on this curious subject, by observing the effects of immersion in fresh and salt water, of equal temperature, on the animal heat. And this might also assist in accounting for the death of the unfortunate men already mentioned.

EXPERIMENT I.

I placed a large vessel, containing one hundred and seventy gallons of salt water, in the open air. The atmosphere was damp, and what is called raw. The thermometer stood at 44° in the air, and this also was the temperature of the water. The subject of my experiment was Richard Edwards, a healthy man, twenty-eight years of age, with black hair, and a ruddy complexion. The hour chosen for his immersion was four in the afternoon, about two hours after his dinner; a time appointed rather for my own convenience, than as being most proper for the purpose. His heat was 98° before undressing, his pulse 10 in the minute. He was undressed

undressed in a room where the mercury was at 56°; and afterwards stood naked before the fire till his heat and pulse were examined again, and found as before. He then walked pretty briskly through a passage paved with stone into an open court, where the north-east wind blew sharply upon him: he was exposed to it for a minute, and then plunged suddenly into the water up to the shoulders. The thermometer, which had been kept in a jug of warm water, at the heat of 100°, was introduced into his mouth, with the bulb under his tongue, as soon as the convulsive sobbings occasioned by the shock were over. The mercury fell rapidly, and a minute and a half after immersion it stood at 87°. He remained motionless in the water, and the mercury rose gradually; at the end of twelve minutes it stood at 93½°. While he set in the water, it occurred to me to examine his heat when he rose out of it into the air: I had reflected on the power that must be employed to keep up his heat in a medium so dense as water, and where an inanimate body, of the same bulk, would have cooled so much more speedily than in air of the same temperature. Supposing that this heat-producing process, whatever it may be, might continue its operations some time after the extraordinary stimulus (the pressure of the water) was removed, I expected to see the mercury rise by the accumulation of his heat, on changing the medium of water for air, and therefore kept him exposed, naked,

naked, to the wind, two minutes after taking him out of the bath. To my surprise, although the attendants were rubbing him dry with towels during this time, the mercury fell rapidly. He was put into a warm bed, and his heat, when examined under the tongue, was 87°, at the axilla, 89°. Frictions were used, and brandy mixed with water administered; but I found on this, as on all future occasions, the best mode of counteracting the cold, was to apply a bladder, with hot water, to the pit of the stomach (the scrobiculus cordis), a fact which I think important: this being done, his shiverings, which before were severe, soon ceased, and he became more comfortable. Three hours afterwards, however, he had not entirely recovered his former heat, but by eight at night he was in all respects as usual.

I have been very minute in detailing the circumstances under which this experiment was made; some of the particulars which, at the time, I thought of little consequence, I found afterwards of importance. The experiment itself I determined to repeat as exactly as possible.

### EXPERIMENT II.

On the next day, at the same hour, the same person was again immersed, as before. His pulse previously

previously was 85, his heat 100°. He had been put to bed an hour before, to save the time spent in undressing. The heat of the water and of the atmosphere 44°. The wind north-east, and strong. On this occasion, as before, there was a rapid fall of the mercury; the following table will save words:

| | Ther. | | Ther. |
|---|---|---|---|
| 2 min. after immers. | 89½° | 9 min. after immers. | 95½° |
| 3 —— . . . | 90½ | 10 —— . . . | 94½ |
| 4 —— . . . | 92½ | 11 —— . . . | 95 |
| 5 —— . . . | 94½ | 12 —— . . . | 95 |
| 6 —— . . . | 95 | 13 —— . . . | 95¾ |
| 7 —— . . . | 95¾ | 14 —— . . . | 95 |
| 8 —— . . . | 95¾ | | |

At the end of fifteen minutes he was taken out, and stood three minutes, naked, exposed to the north-east wind, at the end of which time the mercury had sunk to 88°. A draught of ale was given him, and he was put into a warm bed, in three minutes afterwards the mercury rose to 93°. An hour after his heat was 95°.

The effects produced by this alternate exposure to water and air of the same temperature, gave a new direction to my thoughts, and determined me to inquire again into this singular phenomenon. The most obvious method would have been to have prolonged the process of alternation, and re-

plunged

plunged the person cooled by the external air into the bath; but this was running too great a risque, unless some more sudden and certain method could be found of restoring the heat that might be lost. It was prudent, therefore, to proceed more cautiously. In the next experiment I resolved to try the methods of heating, as well as of cooling the body.

### EXPERIMENT III.

On the following day, at the same hour, the same person was again immersed in the salt-water bath. His heat previously was 98°, his pulse 100. The temperature of the air and the atmosphere, as before, 44°. The mercury sunk rapidly to 90°.

| | Ther. | | Ther. |
|---|---|---|---|
| 2 minutes after . | 88° | 10 minutes after | 94½° |
| 3 ——— . . | 88 | 11 ——— . . | 94¾ |
| 4 ——— . . | 88½ | 12 ——— . . | 95 |
| 5 ——— . . | 90½ | 13 ——— . . | 96 |
| 6 ——— . . | 92 | 14 ——— . . | 96 |
| 7 ——— . . | 92 | 15 ——— . . | 96 |
| 8 ——— . . | 94 | 16 ——— . . | 96 |
| 9 ——— . . | 94 | | |

He was now taken out, and stood in the wind three minutes, shivering violently. This circumstance rendered it difficult to ascertain exactly the fall of the mercury, which was, however, considerable. When he was examined in the room in which

which he undressed, it stood at 90°. He was now plunged into a fresh water warm bath, heated to 97½°. What is very surprising, the mercury fell two degrees.—The following table will shew the progress of the return of his heat.

| | Ther. | | Ther. |
|---|---|---|---|
| 1 min. after immers. in the Warm bath, mercury, . . . | 88° | 5 minutes after, . | 94° |
| 2 minutes, . . . | 92 | 6 ——— . . . | 96 |
| 3 ——— . . . | 92 | 7 ——— . . . | 96 |
| 4 ——— . . . | 94 | 8 ——— . . . | 96 |
| | | 9 ——— . . . | 96 |

If the rise of heat in the cold bath at 44°, and the warm bath at 97½°, be compared, the first will be found more slow; but that after being sixteen minutes in the one and in the other, the heat was the same in both cases, when taken at the mouth. It must, however, be acknowledged, that in the cold bath, the extremities were chilled and cold, while in the hot bath, the heat was equally diffused.—When Edwards got out of the hot bath, he put on his clothes, and was remarkably alert and cheerful the whole evening. Encouraged by the safety of these experiments, I resolved to increase the time of immersion in the cold bath, and to inquire more generally into its effects on the sensations, as well as heat.

EXPERIMENT

EXPERIMENT IV.

At the same hour of another day, the same person was again immersed as before, his heat being previously 97½°, and that of the water 42°. Wind north-east, and brisk.

| | Ther. | | Ther. |
|---|---|---|---|
| 1 minute after, heat | 90° | 12 minutes . . | |
| 2 minutes . . . | 92 | 13 ——— . . | |
| 3 ——— . . . | 92 | 14 ——— . . | 94½° |
| 4 ——— . . . | 92½ | 15 to 24 . . . | 94½ |
| 5 ——— . . . | 92 | 25 ——— . . | 94 |
| 6 ——— . . . | 92½ | 26, 27 . . . | |
| 7 ——— . . . | 94 | 28 ——— . . | 94½ |
| 8, 9, 10, 11 . . | 94 | 29, 30 . . . | 94 |

It will be observed, that in the above table there are blanks left in the report. At such times the thermometer was taken out of Edwards's mouth, to admit of his answering the questions put to him. He said, that on plunging into the water, he felt an extreme cold, which he could not but think was partly owing to his being exposed, naked, to the wind before; that this cold diminished, and in a little while he felt comfortable, but that after a while the sense of coldness returned, though less than at first; diminishing again, but in a less degree. At length his sensations became pretty fixed. In this state, when the water was at rest, he should not even have known, by his feelings from the upper part of his chest to the pubes, that he was in the water at all. His feet and legs were very

cold;

cold; so were his hands and arms; and so also the penis and scrotum. He mentioned, likewise, that he felt a cold circle round the upper part of his body, though not constantly. On examining into this, I found it was greatest at first, and that it extended over the space which, from the undulations left in the bath by the plunge of immersion, was alternately above and under the surface of the water: when the bath settled, it was little felt; but by agitating the fluid, I could reproduce it at any time when the cold in the extremities was not so great as to prevent its being felt. This curious particular serves to explain a circumstance much dwelt on by Mr. Amyat, in giving an account of his sufferings on the wreck; that what he felt most severely was the cramps in the muscles of his hips and sides, parts which, from his situation on the wreck, already described, must have been alternately under and above the surge. Here I must observe, that the sea did not break over them. The wind moderated, as well as the waves, and for the last fifteen hours, they were not at any time overwhelmed, or at least Mr. Amyat himself was not. The cold never abated. Being all lashed to the wreck, they never changed their positions; the bodies of those who died occupied the space where they were originally placed. Mr. Amyat, therefore, during the whole time sat nearly up to the middle in water, but subject to the variations occasioned by the motion of the sea.

To

To return—When exposed naked to the wind, the mercury, in this case, sunk as usual five or six degrees, and his shiverings were great. Desirous of restoring his heat as speedily as possible, we incautiously heated the hot bath to 104°: but after being half a minute in it, he screamed out with pain, especially in his extremities, and about his scrotum. When taken out, his shiverings almost amounted to convulsion. The bath was lowered to 88°, and he was replaced in it, and its temperature progressively, but pretty rapidly increased to 100°. He continued, however, to shiver much, his heat remaining about 90°; but a bladder, with very hot water, being introduced under the surface of the bath, and applied close to his stomach, the good effects were instantaneous, his shiverings ceased, and his heat mounted rapidly to 98°.

All these experiments having been made on one person, I determined to repeat this last on another.

EXPERIMENT V.

R. Sutton, aged 19, of a pale complexion, and a feebler frame, was immersed in the bath, under the circumstances of the preceding experiment. His heat was previously 96½°.

½ a min.

| | Ther. | | Ther. |
|---|---|---|---|
| ½ a min. after, | heat 92° | 18 minutes . . | 93¼° |
| 1 minute . . . | 90 | 19 ——— . . | 93½ |
| 2 minutes . . | 88½ | 20, 21 — . . | 94 |
| 3 ——— . . . | 89 | 22 ——— . . | 92½ |
| 4 ——— . . . | 90 | 23 ——— . . | 92¾ |
| 5 ——— . . . | 92 | 24 ——— . . | 92¼ |
| 6 ——— . . . | 92½ | 25 ——— . . | 94 |
| 7 to 10 . . . | 92 | 26 ——— . . | 94 |
| 11 ——— . . | | 27 ——— . . | 92½ |
| 12 to 15 . . . | 92 | 28 ——— . . | 92¾ |
| 16 ——— . . | 92½ | 29 ——— . . | 94 |
| 17 ——— . . | 93 | 30 ——— . . | 94 |

Though this person seemed to bear the cold bath well, having lost in thirty minutes only 2¼ degrees of heat, yet when exposed afterwards to the wind, he shivered violently, and lost his heat very fast. He was put into a warm bath, heated to 96°, but recovered his heat very slowly, as the following table will shew:

| | | |
|---|---|---|
| 1 minute after, | heat 88° | |
| 2 minutes . . . | 90 | |
| 3 ——— . . . | 90½ | |
| 4 ——— . . . | 90 | great shivering. |
| 5 ——— . . . | 90 | here the bath was heated to 100°. |
| 6 ——— . . . | 90 | shiverings still. |
| 7 ——— . . . | 90 | ditto. |
| 8, 9 —— . . . | 90½ | ditto. |
| 10 ——— . . . | 92 | ditto. |

11 min.

| | | |
|---|---|---|
| 11 min. after, | 92° | bath heated to 104°. |
| 12 ——— | 94 | |
| 13 ——— | 93 | — heated to 108°. Shiverings. |
| 14 ——— | 93 | a bladder with very hot water applied to the stomach. |
| 15 ——— | 94 | |
| 16 ——— | 96 | very comfortable. |

EXPERIMENT VI.

Richard Edwards, the original subject of experiment, was again immersed in the cold bath, of the temperature of 40°, and remained in it three quarters of an hour. His heat previously was 97° his pulse 90 in the minute. The mercury fell at first to 92°, was stationary for a few minutes, and then mounted, though as usual, with no regularity. In twenty-two minutes it stood at 96°; it then began to decline, and in twenty-three minutes more, had sunk to 94°. Being exposed as usual to the wind, the mercury sunk as usual, and he shivered violently. In the warm bath at 96°, his shiverings continued several minutes, his heat remaining at 90° and 91°. In seven minutes the mercury began to rise fast, and five minutes after it was at 96°.

EXPERIMENT VII.

The effects of forty-five minutes' immersion in the cold salt-water bath, at 40°, were proposed to be tried on Richard Sutton. He was much under the impressions of fear, and his heat previously raised the mercury only to 94°. The mercury sunk, as usual, on his immersion, but to an unusual degree. It did not stop on its fall till it got to 83°, which perhaps might in part be accounted for by the extraordinary chattering of his teeth, admitting some contact of the air. It then mounted in the usual irregular way, and at the end of thirteen minutes had got to 92°. Here it stood for nineteen minutes longer, with little variation; at the end of this time it began to fall rapidly, though irregularly, and in three minutes was down at 85°. He had now been thirty-five minutes in the water, and I did not think it safe to detain him longer; we therefore hurried him into a warm bath, heated to 96°, where he shivered much.—The bath was heated gradually to 109°, and in this heat he recovered his proper temperature in about twenty-eight minutes. Being then put into a warm bed, he fell into a profuse perspiration, which left him in his usual health.

One general remark will serve for the pulse in all these experiments. It was not possible to keep

the subjects of them from some degree of previous agitation, and this always quickened the pulse.— The natural pulse of Edwards was about 70 in the minute; but it may be observed, that it was never under 85 before immersion, and generally above it. However this might be, it invariably sunk to 65, or from that to 68, in the water; became firm, regular, and small. After being long in the bath, it could hardly be felt at the wrist, but the heart pulsated with great steadiness and due force. In the last experiment, when the heat sunk rapidly, Sutton said that he felt a coldness and faintness at his stomach, which he had not perceived before, and when I examined the motion of his heart, it was feeble and languid. In some future trials of the effects of immersion in fresh water (one of which I shall detail) the same coldness at the stomach preceded a rapid fall of the mercury; and these facts, together with the effects I found from applying a considerable heat to this part when the body was chilled with cold, convince me that there is some peculiar connexion of the stomach, or of the diaphragm, or both, with the process of animal heat. Whoever will consider the rapidity with which a dead body would have cooled, immersed in water of the temperature of 40°, may form some estimate of the force with which the process of animal heat must have acted in the experiments already recited. These experiments,

ments, however, do not coincide with our generally received theories of animal heat. The increase of heat, in fever, has led some persons to believe that animal heat is produced by, or immediately connected with, the action of the heart and arteries; here, however, it may be observed, that while heat must have been generated in the bath with more than four-fold its usual rapidity, the vibrations of the arterial system were unusually slow. Another, and a very beautiful theory of animal heat, supposes it immediately to depend on respiration; but in the bath, after the first irregular action of the diaphragm from the shock of immersion was over, the breathing became regular, and unusually slow. Lastly, the curious phænomenon of the heat rising, and falling, and rising again, in the bath, with the body at rest, and the temperature of the surrounding medium unchanged, is, I think, fatal to those theories of animation, which consider the living body as a mere machine, acted on by external powers, but not itself originating action, and differing from other machines only in the peculiarity of the powers which are fitted to set it in motion.* I have said that temperature of the

 medium

* I do not mean to object to the theory which refers the introduction of caloric into the system, chiefly to the decomposition of atmospheric air in respiration, but the actual

medium continued unchanged, but it may be supposed that the bath was heated a little during the experiments; it was so; but being exposed, with a large surface, to the open air, the wind blowing briskly over it, its heat was little altered; in twelve minutes' immersion it had gained nearly one degree, and in forty-five minutes, the longest duration of any one of the experiments, it had gained three degrees. As this accession was regular, if it had been greater, it would not have invalidated the foregoing observations.

Many other trials were made on the effects of immersion in water on the human heat, which I shall speak of generally, under the general conclusions which they suggested.

The experiments already recited, suggested to me the notion, that in all changes, from one medium to another, of different density, though of the

---

actual state of heat in the body, cannot safely be inferred (as some have supposed) from the quantity of air respired. Caloric is received into the system by other processes also, particularly from the food and drink in the state of digestion, and evolution of it into active heat appears to be modified by the living power on its own peculiar principles; at least it is not explained by chemical or mechanical principles, or analogies, so far as we are yet acquainted with the subject.

same temperature, there is a loss of animal heat. I found, however, that this conclusion requires many restrictions.

1. My experiments being made on bodies of such very different density as air and water, do not admit an universal inference of this sort.

2. Being all made in a temperature of fifty degrees under the human heat, no certain conclusion can be drawn as to what might happen in degrees of heat much higher, where it is probable, the effects of the change, if it appeared at all, might be less striking. It should seem, however, that after a person is long chilled in cold water, the first effect of passing through the external air into the warm bath, is first a fall of heat in the air, and after this a still greater fall in the warm bath, followed, however, by a speedy rise.

The air and water being equally cold, and both 45° or under, I found the loss of heat in passing from the one to the other to be regulated in the following way.

1. If, instead of being exposed naked to the wind

wind previous to immersion in the water, the body was kept warm by a flannel covering, the mercury fell much less on the plunge.

2. If, after plunging in the water, the person continued in it only a minute or two, a subsequent fall of the mercury did not always take place, on his emerging into the air. On the contrary, there was sometimes a rise on such occasions in the mercury, especially if the atmosphere was at rest.

3. In one instance, after continuing in the water fifteen minutes, in rising into the air in a perfect calm, though during a frost, there was little or no seeming diminution of the heat; while the exposure, under similar circumstances, with a north-east wind blowing sharply, though the air was many degrees warmer, produced a rapid diminution. The effects of the wind in diminishing the human heat are indeed striking, and are not in my opinion explained by the common suppositions.

4. The loss of heat, by a change of media, depends much on the rapidity of the change, for the plastic power of *life*, in varying the process of animal

animal heat, so as to accommodate it to the external changes, acts for a time with great celerity, though this celerity seems to diminish with the strength.

### EXPERIMENT VIII.

I placed in a large room, where the mercury stood at 36°, two slipper baths, at the distance of six yards from each other. One was filled with cold salt water of the temperature of 36°, the other with water heated to 96°, which was my own heat. Undressing myself in an adjoining room by a fire, I afterwards slipped on a loose flannel dress, and descended *slowly* into the cold bath, where I remained two minutes; I ascended *slowly* into the air, and then sunk myself in the warm bath, where I remained two minutes also: I returned to the cold bath, where I staid two minutes as before, and removed from it again to the warm bath. But during all these changes of media and temperature, the thermometer, with its bulb under my tongue, never varied from 96°. I attribute this partly to the heat of my body being in some degree defended by the flannel dress, partly to the calm of the air, but chiefly to the slowness of motion in these changes. It may be said that the time of staying in the different baths was not long enough

to

to produce any sensible change in the heat of circulating fluids of such a mass, but this is not consistent with many of the other facts.

5. The influence of the application of cold water to the surface of the body on the heat, is in some respects regulated by the animal vigour, as the following experiment will show.

### EXPERIMENT IX.

In the same room I placed a large empty vessel, in which two young men sat down in succession, each with the bulb of a thermometer under his tongue. A man standing on a bench with a bucket of cold salt-water containing four gallons, poured the whole on the head and shoulders, suffering it to run down on the rest of the body. This process took up nearly a minute, during which I examined the mercury, and found it unchanged. They were both directed to continue sitting without motion for a minute after, during which, in both instances, the mercury rose two degrees. A third, much inferior in vigour, submitted to the same experiment, and the mercury continued during the affusion of the water unchanged, but in a minute after sunk half a degree. In fevers, where the heat is generally increased from two to six degrees above the standard of health, pouring

pouring a bucket of cold water on the head, always reduces the pulse in frequency, and commonly lowers the heat from two, to four or five degrees. Of this salutary practice I hope soon to speak at large to the public*.

6. The power of the body in preserving its heat under the impressions of cold, and the changes of temperature, and of media, seems in some measure regulated by the condition of the mind. That fear increases the influence of cold, and of many other noxious powers, will not be doubted; but the state of the mind to which I allude, is that of *vigorous attention* to other objects. This, it is well known, will to a certain degree deaden, or, indeed, prevent the sensation of cold; and what does this, I apprehend, prevents, or at least weakens, its physical action. The astronomer, intent on the objects of his sublime science, it is said, neither feels, nor is injured by, the damps or the chillness of the night; and in some species of madness, where the ideas of imagination are too vivid to admit the impressions of sense, cold is resisted to an extraordinary degree. I have seen a young woman, once of the greatest delicacy of frame, struck with madness, lie all night

* Written in 1791.

night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that she was to feed on was a mass of ice.*

7th. There are particular conditions of the atmosphere, not perfectly understood, that seem to have an influence in depriving us more speedily of our animal heat, than others where the cold is greater.

It may seem, that by this time I had renounced my intention of trying the effects of immersion in fresh water on the animal powers, and particularly on the heat. Some trials, I have, however, made, of which I shall only relate the following.

EXPERIMENT X.

In the same vessel, containing an equal bulk of fresh water, Richard Edwards, the subject of my first experiments, was immersed, at the same hour of the day. His heat previously was 98°, his pulse beat 92 in the minute, the heat in the air was 41½°, that of the water 40°. The wind was now in the west, so that in the court where the bath

* This power of resisting cold is not however general in insanity.

bath stood, there was a perfect calm. As I had some fears of the issue of this experiment, instead of exposing him for a minute naked to the wind before immersion, he was covered with a flannel dress from the air, till the instant he descended into the water, into which he was suffered to sink himself slowly, with the bulb of the thermometer under his tongue. These are important circumstances. The following table exhibits the result.

| | |
|---|---|
| Immediately on immersion heat . . 98° | 14 minutes after 96½° |
| 1 minute after, . 97½ | 15 ——— . . 96 |
| 2 minutes . . 97 | 16, 17, 18, 19, 20, 96 |
| 3 ——— . . . 98 | 21, 22, 23, 24, |
| 4 ——— . . . 97½ | 25 ——— . . 95 |
| 5 ——— . . . 96 | 26 ——— . . 94 |
| 6 ——— . . . 96 | 27 ——— . . 93½ |
| 7, 8 . . . . 96 | 28, 29 . . . 94 |
| 9 ——— . . . 97 | 30 ——— . . 93 |
| 10 ——— . . 97 | 31, 32 . . . 94 |
| 11, 12, 13, . . | 33, 34, . . . 92½ |

He now got out into the air very slowly, and stood in it three minutes, the wind *not* blowing on him. He lost one degree of heat at first, which he recovered. He was then put into a warm bath at 90°, which at first *he felt* warm, and his feet and hands were pained, but in two minutes he fell into a very violent shiver, and his heat fell two degrees. The bath was then heated to 95 and 96°, but still he felt cold. It was heated to 99°: he continued

continued in it five minutes, and his heat was 91°. The heat was gradually raised to 106°, when the sense of coldness, of which he had complained at the pit of the stomach, gradually went off. Before this I had usually kept him in the warm bath till his natural heat was nearly recovered, but after being half an hour in the heat of 106°, his own heat was still 93°. He now became sick and very languid, a cold sweat covering his face, his pulse very quick and feeble. He was removed into bed, but passed a feverish night, and next day had wandering pains over his body, with great debility, resembling the beginning stage of a fever. By cordials and rest this went off.

This experiment clearly enough confirms the greater danger of being wet with fresh than salt-water; but in itself points out nothing certain besides, except that it is not to be rashly repeated. I mean to try some of these experiments to a greater extent on the brute creation, when I have procured thermometers better suited to my views. The thermometers I employed had not a sufficient mobility for very nice experiments, and I am well aware, that in particular instances, this may have misled me, though the general results, which is all that is of importance in such experiments as these, will, I hope, be found just and true.

Before

Before I conclude, I must offer a few observations on the subject that led to these experiments.

1. It is, I think, already well known among seamen, that where there is only the choice of being wet with salt or fresh water, it is always safest to prefer the first. In the heavy showers of rain, hail, or snow, by which gales of wind are generally accompanied, the men that must be exposed to them, ought, like Lieutenant Bligh and his crew, to wring their clothes out of salt-water.

2. In all cases where men are reduced to such distress by shipwreck or otherwise, that they have it only in their power to chuse between keeping the limbs constantly immerged in the sea, or of exposing them to the air while it rains or snows, or of being exposed to it where the sea is at times washing over them, it is safest to prefer a constant immersion; because, in the northern regions, where the cold becomes dangerous to life, the sea is almost always warmer than the air, as the experiments of Sir Charles Douglas shew; and because there is not only a danger from the increased cold produced by evaporation, but also from the loss of heat by the rapid changes of the surrounding medium, as the foregoing experiments point out.

3. Whether, in high and cold winds, without rain

rain or snow, and where a situation may be chosen beyond the reach of the waves, it is safer to continue in the air, or to seek refuge in the sea, must depend upon several circumstances, and cannot perhaps be certainly determined. The motives for choosing the sea will be stronger in proportion as the wind is high and cold, and in proportion as the shore is bold.

The foregoing narrative shews, that men may survive twenty-three hours' immersion in the sea, of the temperature of 38° or 40°* (as great a cold as it almost ever possesses) without food or water, and almost without hope of relief; but that any man ever survived an equally long exposure to the higher degrees of cold of the atmosphere, in the same circumstances, does not appear. Though in the case related, immersion in water did not prevent thirst, yet there is little doubt that it alleviated it, a circumstance of high importance towards the preservation of life.

I have purposely avoided any reasoning on the cause of the loss of vital heat, on the change of media in the experiments recited. It may be supposed that during immersion, the water immediately

* These numbers ought I think to have been 33° or 35°. See note, p. 195.

ately in contact with the skin, having become heated to a certain degree, the naked body, on rising from it into the air, was in fact exposed to a colder medium, and thus the loss of heat, in this instance, produced. My examination of the heat of the water during immersion, not having been made in contact with the body, I will not deny, that there is some foundation for the remark; and the cases, it must be allowed, are by no means exactly parallel between immersion in an open vessel, however large, and immersion in the sea, where the constant undulation may be presumed to occasion a continual change in the surrounding fluid. But whatever allowance may be made for the circumstance mentioned, I am persuaded, that the difference between the density of air and water being considered, it is not sufficient to explain the loss of heat in the instance alluded to. The changes of temperature in the living body are governed by laws peculiar to itself. I have found, in certain diseases, greater and more sudden variations than any mentioned, from applications of cold, very gentle in degree, and momentary in duration.

In his masterly "Experiments and Observations on Animal Heat," Mr. Hunter has objected to taking the heat of the human body by introducing the bulb of the thermometer into the mouth, because it may be effected by the cold air in

in breathing. The objection is well founded if the bulb be placed on the upper surface of the tongue, but if it be put under it, and the lips shut, the effects of respiration may be disregarded, as I have found from many hundred experiments. The heat may be observed in this way with ease and certainty, by employing thermometers curved at that end to which the bulb is affixed (the bulb being introduced at the corner of the mouth) some of which have been made for me by Mr. Ramsden, according to a form given, as well as others on Mr. Hunter's plan. From repeated trials it appears to me, that when the usual clothing is on, the heat of the living body may be taken with nearly the same result, and equal certainty, under the tongue with the lips shut, at the axilla with the arm close to the side, and in the hollow between the scrotum and the thigh; every other part of the surface is liable to variation and uncertainty. It is evident, that of these three methods, the first only can be employed (as far as I can discover) when the trunk of the body is immersed in water; and even when the naked body is exposed to the cold air, the first method seems the best, the heat remaining most steady under the tongue; the axilla is the next best in order, and the worst the lower part of the groin; for the scrotum, and the parts of generation, lose their heat on the application of cold, more speedily perhaps than

than any other part of the body, the extremities not excepted.

The water employed in the experiments related, contained salt in the proportion of one to twenty-four.

1803. It was my intention to have repeated and enlarged these experiments, but the difficulty attending them, and the pressure of professional engagements, has hitherto prevented me.

The two last chapters have carried us off from the subject of fever, to which we are about to return; it is hoped, that besides illustrating some of the more general effects of cold on the living body, they may assist us in a few observations on the nature of fever, and the mode of operation of the cold affusion, which it is the chief object of this publication to recommend.

# CHAP. XVII.

*General view of doctrines respecting fever.—Hippocrates—Galen, &c.—The ideas of the author respecting the nature of this disease.*

THE great obstruction which men have in all ages experienced in the pursuit of knowledge, has arisen from the promptitude of the human mind to decide in regard to causes. To the weak and ignorant, presumption is as natural, as doubt is intolerable, and with such, belief is almost always a creature of the imagination. Nor do these observations apply to weakness and ignorance only: to retain the mind unprejudiced and undecided, in the investigation of striking and interesting phenomena, till, by the painful steps of induction, their hidden cause is revealed, is an effort of the most difficult kind, and requires the highest and rarest powers of the understanding. The records of every part of science bear ample testimony

testimony to this truth, particularly the records of medicine, and in a still more especial manner, that part of medicine which treats of the nature of fever. The most eminent physicians, in every period of the world, impatient of observing and delineating, have been eager to explain, and even to systematize; and the science of life owes its corruptions more to the misapplication of learning, than even to the dreams of superstition. Besides the theories derived from the splendid fictions of the Greek philosophy, various are the false doctrines introduced into medicine in modern times, and from the more certain branches of science. The principles of mechanics, of chemistry, of magnetism, and of electricity, have in succession been employed to explain the motions of life, and have served only to mislead and to deceive.

Hippocrates, who lived at too early a period to be acquainted with the collateral branches of science, studied life and disease in the book of nature, and had the merit of an original observer. Perceiving the increase of heat to be the most remarkable symptom in fever, he assumed this for the cause, and founded his distinctions of fevers, on the different degrees of the intenseness of this heat. He had not an instrument that could measure this exactly, and necessarily

trusted to his sensations. In forming his diagnosis, he placed his hands on the breasts of his patients, depending more to the degree of heat, than on the state of the pulse, the nature and connexions of which he did not understand. His practice appears to have been natural and judicious, and founded on his theory. He directed linen, dipped in cold water, to be applied to the hottest parts; drew blood away both by cupping glasses and the lancet; and administered cold water and cooling drinks, particularly barley water and honey. This simple practice will acquit the Coan sage with the candid and reflecting, of many of the idle theories which have been imputed to him, and which doubtless were the offspring of after times.

In the days of Galen, philosophy and science had made some advances, and the corruptions which he introduced into medicine from those sources are well known. Preserving however the doctrine of his master, that heat was the cause of fever, the practice of Galen, founded on this notion, was in some respects bold and successful, as was before observed. To him succeeded the Arabians, who still retained the original doctrine of Hippocrates, but with new modifications and corruptions, by which the real nature of fever was still farther obscured.

In

In our own country, and in the last century, arose Sydenham. He also was an original observer, and to him medicine is doubtless much indebted. That he recorded symptoms with great accuracy, that he established many important facts as to the treatment of disease, and that he was a more cautious reasoner than his predecessors or his contemporaries, are points that are indisputable. But though he affected not to theorize, he was a theorist in every page of his works, and though he founded his reasonings on what appeared to him self-evident principles, yet time has proved them to be defective and fallacious. It was the postulate of Sydenham, that every disease is nothing else but an endeavour of nature to expel morbific matter of one kind or another, by which her healthy operations are impeded. In this endeavour she is not to be obstructed, but assisted, and the process is to be carefully watched and promoted by which she accomplishes her purpose. By one or other of the emunctories this is finally effected, and till it be effected, health cannot be restored. Under this general notion, the inordinate actions of fever are perpetually compared to the motions of fermentation, by which nature separates the vitiated particles from the blood, previously to their expulsion. The doctrine of Sydenham may be traced to remote ages; under his sanction it spread over Europe,

Europe, and has descended in one form or other to our own times. Sydenham's doctrine introduced the system of Boerhaave, (to which we have already alluded) as well as other chemical theories of lesser fame, and the erroneous modes of treatment to which they gave birth. The inordinate heat, which Hippocrates considered as the cause of fever, in the eye of the chemists, appeared naturally enough, a necessary, and within certain limits, a salutary consequence of the process excited by nature to expel the disease!

The dawn of a juster pathology of fever is to be found in the works of Hoffman. Though he also undertook to be the interpreter of nature's intentions, he contemplated her process in fever with more sagacity, and rejecting chemical and mechanical analogies, endeavoured to discover the cause of fever in the peculiar nature of the vital motions. He supposed the noxious cause producing fever (in the language of the schools the remote cause) to operate first on the living solids, occasioning a general spasm of the nervous and fibrous system, beginning in the external parts, and proceeding towards the centre. In consequence of this, a contraction of the vessels of the extremities must of course take place, impelling the circulating fluids in an increased ratio on the heart or lungs; which, stimulating these organs

organs to increased action, the fluids are thereby repelled towards the extremities, and thus the phenomena of fever are produced. There are therefore, according to Hoffman, two distinct sets of motions in fever, the first from the extremities towards the centre, arising immediately from the spasm, and accompanied by a small pulse, anxiety, and oppression; the second, from the centre towards the surface, which is the effort of nature to resolve this spasm, and marked by a full, strong pulse, and increased heat. The first of these sets of motions are baneful, and sometimes fatal; the second are medicinal and salutary. By these views the physician is to be directed in counteracting the morbid actions, and assisting the sanative process of nature.*

Since to theorize seems to be the irresistible propensity of men of genius, this theory of the celebrated Hoffman, which accords with appearances, and has a considerable connexion with practice, may be allowed the tribute of applause.

The system of Hoffman, produced that of Cullen, which may perhaps be considered as the prevailing doctrine at present, when however, theory

* See Hoffman's Works, vol. i. tom. ii. page 10.—*Geneva Edition.*

theory of every kind has lost much of its weight on mature understandings. According to Hoffman, the first effect of the remote cause of fever is the spasm, producing a re-action, as has already been mentioned. Dr. Cullen introduced a previous link into the chain—He contended, that the first effect of the noxious effluvia (the remote cause) was a general debility, affecting the sensorium commune. To this debility, he attributed the spasm, and to the spasm the re-action of the heart and arteries; which re-action, continuing till the spasm is resolved, removes the debility and the disease. According to Hoffman, the spasm belongs to the class of motions that he denominates baneful; but Dr. Cullen presumes it to be salutary, and therefore ascribes it, in the language of the schools, to the *vis medicatrix naturæ*. It is not my purpose to appreciate these changes introduced into the theory of Hoffman, or to attempt to estimate with precision, the Cullenian doctrine of fever, as it arranges phenomena, or applies to practice. Debility of a peculiar kind, spasm, and re-action of the heart and arteries, seem all of them links in the chain of fever, and in the first lines of Dr. Cullen, the history of the disease is recorded with extraordinary minuteness and accuracy. I bow with respect and sensibility to the genius of this illustrious man; but his

doctrine

doctrine seems erroneous, in as far as it enters into the supposed intentions of nature, and defective, in passing over the morbid heat, and the morbid association, which form the successive links of the chain.

This doctrine of fever, as well as all the other doctrines of its celebrated author, was assailed by Dr. Brown. Assuming the existence of an unknown principle, as inherent in the living fibres, to which he gave the name of excitability, he explained all the phenomena of life and disease, by means of this principle, acted on by stimuli. These stimuli, applied in the due proportion, produce the just degree of excitement, that is, the state of health. If the stimuli are diminished below the healthy proportion, he supposed the excitability to accumulate; if increased beyond this proportion, he supposed it to be expended; and on these different conditions he attempted to found a general theory of disease. Diseases he divided into two classes, Sthenic and Asthenic, or diseases of increased and of diminished excitement, in the latter of which classes he placed typhus, the fever of which we treat. In the excitability, Dr. Brown admitted of no change, except in regard to quantity; in the excitement, no variation, except in regard to strength; and in all universal diseases, he

he supposed the whole system to be equally affected.* Having therefore assigned to fever its place in his series of descending excitement, he refused to inquire into its symptoms, or to enlarge on its treatment. It is evident, that such a theory could embrace none of the characteristics of the disease. Debility, the first link in the chain of Dr. Cullen, formed according to Dr. Brown, the essence of fever. The existence of spasm he denied, re-action he derided, and the morbid heat and morbid association, he wholly overlooked. It is not however to be disputed, that his general views of disease had a salutary influence on the practice in typhus, by encouraging a more liberal use of wine and opium, remedies of the utmost importance, and now universally adopted.

The various modes of action of the living principle, or to use his peculiar language, of the sensorial power, which were unknown to Dr. Brown, have been observed and illustrated by the author of Zoonomia.

In the theory of fever delivered by Dr. Darwin, the influence of morbid association is very fully explained. I am not however disposed to adopt

* This was his general position; it is true he relaxed a little from it in his explanations.

adopt this theory, which the learned and candid author himself considers as incomplete, because it assumes the truth of his peculiar doctrines, to which it cannot be expected that a hasty assent should be given—considering their vast extent and importance, and considering also, that many other fabrics of genius of the same kind, have crumbled down into the sand of which they were formed.*

Dismissing

* "What I have thus delivered, I beg to be considered "rather as observations and conjectures, than as things ex- "plained and demonstrated; to be considered as a foundation "and a scaffolding, which may enable future industry to erect "a solid and a beautiful edifice, eminent both for its simplicity "and utility, as well as for the permanency of its materials "—which may not moulder, like the structures already "erected, into the sand out of which they were composed; "but which may stand unimpaired like the Newtonian philo- "sophy, a rock amid the waste of ages!" *Zoonomia, vol.* ii. *p.* 625. Much as I admire the singular ingenuity of Dr. Darwin, the position common to him and Dr. Brown, that the sensorial power, or excitability, is a substance which accumulates and diminishes in the inverse ratio of the stimulation, on which his doctrines of fever are founded, has not my assent. On the contrary, it appears to me, that whatever the nature of the vital energy (the sensorial power or excitability) may be, it does not act in a way that admits the relations of quantity to be applied to it, and the contrary doctrine, which introduces into the system of Dr. Darwin so many epicycles, seems to me the mortal part of his work. I am aware that Dr. Darwin holds the doctrine mentioned with certain modifications; which however do not appear to me satisfactory.

Dismissing therefore, as far as it is possible, all theories from the mind, let us briefly consider the process of nature in fever, and examine what inferences may be safely drawn from the succession and catenation of the leading symptoms, and the remedies which experience has pointed out as most successful. Our observations must of course be considered as applying to the typhus, or low contagious fever of Britain, more particularly, but generally to all idiopathic fevers; and if they should be found as little satisfactory in their result, as the conclusions of others, the learned and candid reader will not, it is hoped, suffer his opinion of the accuracy of the author's facts to be affected by the fallacy of his reasonings, but extend his forgiveness to one other abortive attempt to illustrate the nature of fever, an important, but difficult and long contested subject.

Medical science has not ascertained the various remote causes which may produce fever, and I avoid this inquiry, on which I have nothing new, that is satisfactory, to offer*. The first symptom

* In confining the remote causes of fever to contagious human effluvia, and to marsh miasmata, Dr. Cullen probably simplified too far. Cold, under certain circumstances, seems to produce fever, and the constitution seems at times to fall into fever from unknown and internal causes.

symptom of the disease is a debility or languor, very distinctly expressed on the countenance, followed by paleness, sense of cold, and tremor; the functions of the mind are weakened in a correspondent degree with those of the body, and all the actions of the system seem enfeebled. This general debility is peculiar in its kind. The symptoms resemble those produced by inhaling certain gases, whose properties are known; and this circumstance gives some support to those notions of the nature of contagion, which the new chemistry has suggested. They seem also to have a resemblance, though a more remote one, to the effects of certain poisons operating on the stomach, or introduced into the circulation by the absorbents. The remote cause of fever itself, may perhaps be considered as a poison, acting directly on the sensorium commune. Where this poison is concentrated very much, and highly malignant, or where the system is much debilitated, the powers of life are sometimes oppressed and extinguished in the first stage of the disease. In general however, a re-action or resistance commences; the heart and lungs are roused into increased exertion by the pressure of the accumulating fluids, and repel them back on the surface and extremities; while a spasmodic, or morbid stricture of the extreme vessels, opposes the reflux of the fluids, and thus maintaining the inordinate pressure on the centre,

centre, excites the heart, arteries and lungs, to still more violent exertions. In this contest the stomach is usually brought into sympathy, and nausea and vomiting are often induced.

This constitutes what appears to be a struggle between the living energy, and the morbid cause —between the power of the centre, and the resistance of the extreme vessels—in the course of which, a degree of preternatural heat is generated, and the phenomena of the hot stage are produced. When the powers of life prevail, the stricture on the extreme vessels, and on the exhalants of the skin, at length gives way, and a profuse perspiration being poured out on the surface, the heat is carried off, and the febrile symptoms subside. Without entering into detail, this may be considered as a general view of the single paroxysm. It often however occurs, that the solution of the stricture on the extreme vessels does not take place, or is incomplete, and that the morbid heat is not carried off. In this case the morbid actions go on, and the disease runs into continued fever. For some time the stricture on the extreme vessels remains, and the heat is preternaturally great; but these symptoms do not continue through the whole course of the disease. On the contrary, the heat towards the latter stages, will sometimes be found as low as the natural standard, sometimes lower, and

and the capillaries of the skin be completely relaxed; while the inordinate action of the heart and arteries continues, being carried on, as it seems to me, by the morbid association produced in the course of the disease, which retains its influence in this, as in other cases, after the cause that produced it, ceases to operate.

To this general view of fever, it is probable that little objection will be offered, by those who are familiar with the disease. But exceptions may be taken to the language, as not sufficiently clear of theory, against which the author had declared. It may be said, for instance, that in applying the active forms of speech to the re-action or resistance of the constitution, we seem to run into the Stahlian doctrine, or at least to assume the existence of the *vis medicatrix naturæ* of Dr. Cullen; and that to enumerate spasm as a link in the chain of fever, is in fact to admit the leading peculiarity of his system. To this it may be replied, that there is no subject on which the imperfection of language is so much perceived, as in our attempts to describe the phenomena of life. In the strictness of speech we properly employ the passive mood of our verbs, in recording the phenomena of inanimate matter, and confine the active mood to those vital motions, which are accompanied with consciousness or design; but there are various actions

actions peculiar to life which are not accompanied by consciousness, or subject to the will, and which depend on laws wholly distinct from those which regulate inanimate matter. If we borrow the phraseology and the *mood* in which we speak of inanimate matter in recording these actions, we are apt not only to form indistinct conceptions of their nature, but to apply the principles which regulate inanimate motions to the motions of life. This is an obvious, and as experience teaches us, an ample source of error. If, on the other hand, we employ the active forms of speech, these immediately suggest consciousness and design, and the imagination forms to itself an ideal being, as directing these actions, in whom consciousness and design may reside. This also is a natural and fruitful source of error.—Both these extremes we would avoid. But the penury of language obliges us to use one or the other mood in treating of the phenomena in question ; and on reflection it appears, that in our present ignorance of the first principles of life, a clearer notion will be formed of the nature of those motions in which the whole system sympathizes, destitute though they be of consciousness and design, by the occasional use of the active forms of speech, since they have a much stricter analogy with those vital actions, to which these forms are justly applicable, than with the motions of inanimate matter. Considerations of this kind

kind may have influenced the late John Hunter in the use of his terms, which are chiefly *active*, and which, though far from perfect in themselves, are more happy than those of former physiologists, as his doctrines of life were more original, more unprejudiced, and in general more accurate and profound.

*Debility of a peculiar kind, is then the first operation of the remote cause producing fever—the necessary consequence, or as some contend, the concomitant effect, is a spasm, or contraction of the arteries; but more especially of the extreme vessels, and the capillaries of the surface—hence follows an accumulation of blood on the heart and lungs—the re-action of those organs—the generation of morbid heat—and of morbid association.* On each of these links in the chain of fever, let us offer a few remarks.

1. It has been asserted by some inveterate theorists, that the debility in fever has nothing in it peculiar. It is enough to reply, that simple debility often exists even in extreme degrees, without producing the phenomena that constitute fever.

2. It has been asserted, that a spasm on the extreme vessels does not exist in fever, or that if it

does exist, it may safely be disregarded. Those who hold the first opinion, suppose the paleness and shrinking of the surface to arise merely from the enfeebled circulation, by which the blood is not propelled into the extreme parts; and doubtless this has its effect. In syncope, however, where this paleness and shrinking arise from pure debility, as soon as the heart and arteries recover their energy, the blood flows immediately back into the extreme vessels as before. That this is not the case in fever, the most cursory observation will convince us. After the re-action of the heart and arteries has been some time established, the blood is indeed propelled through the vessels of the surface that circulate the undivided fluid; but the capillary arteries that separate the aqueous part, remain constricted, as well as the orifices of the skin that pour it out on the surface.

3. It is however contended, that this stricture may be disregarded, since it is now very generally admitted, that the perspirable matter is not excrementitious, and since we find that its obstruction in other circumstances, and from other causes, produces little or no injury to the constitution. A little reflection will serve to refute this position. Spasm, or morbid stricture on the extreme vessels, must necessarily affect the condition of the heart and lungs, from the increased

pressure

pressure of the circulating fluids on these organs: it must also affect the stomach, not only from the connexion of that organ with the heart and lungs, but from its direct sympathy with the surface.—Dr. Cullen has put these points beyond controversy. But this is not all; the obstruction of perspiration—of profuse perspiration—in the paroxysm of fever, obstructs the process by which the constitution expels the morbid heat, and thus leaves the system under the influence of a general stimulus of the most powerful nature. It was a position of the celebrated Boerhaave, that the morbid heat in fever, being a symptom only, might therefore be disregarded.—But can we suppose, that a heat, six or seven degrees greater than that of the blood in health, however generated, will not have the most important effects on the system, and if it stands in the relation of effect to the preceding symptoms, that it will not operate as a cause on those which succeed?—Doubtless this morbid heat re-acts on the vascular system; irritates the spasm of the extreme vessels; and prolonging the increased action of the heart and arteries, establishes a morbid association, which carries on this increased action after the spasm has relaxed, and the heat itself subsided.

4. What then, it may be inquired, is the actual state of the animal heat in the different stages

of the paroxysm of intermittent, as measured by the thermometer? In different trials it seemed liable to considerable variety, but the following is the general result, which however the reader will receive, as subject to the corrections that a more extensive experience may suggest. In the cold stage, the heat is diminished, not on the surface only, as some have imagined, but very probably over the whole system. I have found it under the tongue, and at the axilla, as low as 94°, 93°, and 92°; and on the extremities many degrees lower. At this time the stomach feels cold, and universal tremor takes place. Warm, and even hot liquids, are highly grateful, and the warm bath, heated to 100°, more grateful still. The drinking of warm liquids should be freely indulged in, and there is reason to believe, that immersion in the warm bath, as practised by Galen, would be found as salutary as it is grateful. By degrees the pulse increases in frequency and force, and the heat begins to rise, but with much irregularity. It doubtless accumulates first in the centre of the system, and from the centre is pushed along with the blood, towards the surface. This process does not however go on in any regular progression, but like almost all the other animal processes, with alternate increase and relaxation. At times it appears retrograde; and even when it is so far advanced that the heat taken at the axilla, and under the tongue, is greater than

than the standard of health, a slight accession of external cold will produce a general chilliness, and bring back the oppression on the heart and lungs. On the surface itself the restoration of heat takes place with no regularity. Certain parts are heated first. In certain parts the heat is above what is natural, while in others, it remains below this standard; and hence arises that mixed sensation of heat and cold, which every one acquainted with fever must have experienced in the transition from the cold to the hot stage of the paroxysm.

The irregularity with which the heat of the surface is restored, probably arises from this circumstance, that two causes conjoin in producing this effect—the reflux of warm blood from the centre, and the sympathy between the stomach and skin. To this last is attributed the circumscribed heat of certain parts of the surface, while the rest remains cool, this local heat being probably excited by the increased action of the vessels of these parts, awakened by the restored heat and tone of the stomach, which in other circumstances produce similar effects. In general the sense of cold predominates even after a morbid heat has taken place at the axilla, under the tongue, and in different parts of the abdomen and thorax; because a considerable part of the surface, and the whole of the extremities, still remain below the natural temperature, and the sensation

sensation which this produces, arising from parts of great sensibility, overcomes the sensation of heat from the deeper seated parts, whose sensibility is so much inferior. At length however the heat of the surface becomes general and uniform, rising to 102°, 103°, 104°, and sometimes 105° of Fahrenheit. I have not seen it higher than this last degree in the paroxysm of intermittent, and seldom in continued fever; though different authors speak of febrile heat four, or even five degrees higher. The sensation of heat is now strong and steady, and the accession of external air does not produce a chilliness as before.—This is the time for the effusion of cold water, as has already been mentioned. The sensation of heat is most powerful on the extremities, particularly on the palms of the hands and the soles of the feet.—This arises in some degree from the great sensibility of these parts, but in some degree also from this circumstance, that in their natural state they are two or three degrees cooler than the trunk of the body, and cannot be raised to the general temperature of health without an uneasy sense of heat, which is frequently relieved by a partial perspiration. When raised to the general fever heat, their temperature is therefore increased in more than a proportional degree beyond its usual standard.

After

After remaining some time in this hot stage, the skin begins to relax, and to become softer and smoother to the touch: insensibly a moisture takes place, terminating generally in a profuse sweat.—Neither however does this process go on regularly; after the moisture commences, it sometimes goes off, and a dry burning state of the skin returns: again the sweat commences, and when it becomes general, and continues steady, it is critical, that is, it terminates the paroxysm. As it commences, the heat declines, though at first very slowly; as it proceeds, the heat subsides gradually, and when it terminates, the temperature of health is restored. The sinking of the morbid heat is however a good deal regulated by the quantity of the bed-clothes, and the closeness with which the body is enveloped.—Where the whole covering that was laid on during this cold stage is kept on during the hot stage, (as often happens, through a mistaken wish to force the sensible perspiration) the heat is carried off with difficulty, and it requires the perspiration to be very profuse to reduce it to its natural standard. On the contrary, if the clothes be lessened after the hot stage is fairly established, the heat passes off with less difficulty, and the perspiration requisite to remove it is much less profuse.

These details will not be thought superfluous or tedious

tedious by those who consider, that after all that has been written on the subject of fever, thermometrical observations on the rise and progress of febrile heat are in a great measure new*.

5. Whether it be owing to the nature, or force of the contagion, to the state of the constitution, or to some other unknown cause, the re-action of the system does not always resolve the spasm or morbid stricture on the extreme vessels. When this stricture is not resolved, the sweat is not effused, the morbid heat is not carried off, and the diseased actions run into continued fever.

If a person is confined in a hot room, or in the hot bath, till his heat rises three or four degrees above the natural standard, his pulse will be found of a feverish rapidity; wandering pains will soon be felt over the body; languor, lassitude, and at length great debility will take place, with most of the symptoms of regular fever. It is evident that these symptoms cannot be expected to go off till the inordinate heat is removed, and if the person remain some time in the heated medium, he will find

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* I intended to have introduced here one or two registers of the heat and pulse, taken every half hour during the paroxysm of intermittent, but this is delayed till I am enabled to speak from more numerous observations.

find, that the inordinate action of the heart and arteries continues after leaving it, and even after his own heat has subsided to its natural standard. This depends on a principle peculiar to life, to which the name of habit, or association, has been given, and which extends its influence to all the vital phenomena, whether intellectual or corporeal.

In fever this morbid heat does not arise from the surrounding medium, but from certain motions in the system itself. But however generated, a heat five or six degrees above the natural standard must be a powerful agent, and it cannot be expected that the diseased actions should subside under so strong a stimulus. The rapidity of the circulation, and the labour of respiration, are consequences of a heat of this degree from whatever cause arising, and must continue till the heat is reduced.

We may here observe, that it is not by any means easy to make an accurate comparison between the state of the system under the heat of fever, and under an equal degree of heat introduced from the surrounding medium, because it is in fact very difficult to raise the heat of the body in a state of health to the highest degree of fever heat, either by heated air or heated water.

The

The experiments recorded by Sir Charles Blagden prove this in respect to a heated atmosphere; the heat, as it is accumulated in the system, being dissipated by profuse perspiration, or by some other living process, of which perspiration is a concomitant effect. I have found the same difficulty in heating the body in the hot bath, for there also the heat is with difficulty increased beyond 100° or 101°, when sweat, as is usual, breaks out over the surface. Even in these degrees the greater part of the symptoms of fever will be felt. The heat however is moderated, and, when the temperature of the external medium is reduced, it is carried off by the discharge from the surface. But in fever this is prevented, for the peculiar debility induced by the remote cause, occasions, or is attended by, a spasm, or morbid stricture of the capillaries of the surface and of the skin itself, by which the insensible perspiration is prevented from increasing in proportion to the heat, and the sensible perspiration obstructed. Hence the difficulty of reducing the heat in fever; and thus it appears, that the spasm on the extreme vessels, which some of our latest physiologists have wholly neglected, is perhaps the most important peculiarity of the disease.

It may be observed, and it furnishes a subject of interesting inquiry, that this stricture, which often

often gives way under a moderate re-action, maintains itself with rigidity where the re-action is great. Thus, where the heat rises to 99°, 100°, or 101°, the orifices of the skin often relax, and carry it off by increased perspiration; while at 104° and 105°, with a proportionable force of circulation, they are in general obstinately constricted. This is very striking in fever, but not peculiar to it. Even in the state of health, while a moderate, gradual, and general stimulus from heat opens the pores of the skin, a sudden and considerable increase of heat constricts them; the violence of the stimulus, in this, as in other instances, occasioning a morbid resistance of the living fibre on which it acts. This resistance to inordinate stimuli extends to every part of the living system, and belongs to every species of vital action—it may be considered as peculiar to life.

But, it may be justly remarked, that after fever is established, the reduction of heat to the natural standard, however desirable, is attended with difficulty and hazard, since the patient cannot in general bear the continual exposure to external cold necessary for this purpose.

Even in the hot stage of fever, if the patient be exposed naked to a cold atmosphere, though he supports the application for some time with comfort,

fort, and even with advantage, yet, as the heat of the surface approaches the standard of health, a sudden sense of chilliness comes on, with a return of oppression on the heart and lungs, and all the symptoms of the first stage of the paroxysm. The length of application, and the degree of external cold producing this effect, are indeed various in various constitutions, and in different stages of the disease: the degree of reduction of the animal heat at which the chilliness commences, is also uncertain; but the general truth of the position laid down is established on ample experience, and will not, I believe, be controverted. In cooling the surrounding atmosphere in fever, or in any other continued application of cold, care must be taken to keep it within the limits in which it is grateful to the sensations; and within these limits the cool treatment, as far as my observation extends, is uniformly advantageous. But though it moderates re-action, it does not in general remove the spasm on the capillaries, or break the association or habit by which fever is prolonged.

This is effected by the sudden affusion of cold water over the naked body; and the mode of its operation will now be easily understood. The sudden, general, and powerful stimulus given to the system, dissolves the spasm on the extreme vessels of the surface, and of the various cavities of the body;

body ; the sudden and general evaporation carries off a large portion of the morbid heat accumulated under the skin, and the healthy action of the capillaries and exhalants being restored, the remaining superfluous heat passes off by sensible and insensible perspiration. The stimulus of morbid heat and of morbid stricture being removed, the morbid association seems also broken by the sudden and powerful impression on the sensations—in fact, the inordinate action of the heart and arteries subsides, and the harassed and toil-worn patient sinks into that peaceful sleep which nature has provided as the solace of our pains and sorrows, and the restorer of our strength.

If such be the explanation of the diminution of the heat, and of the frequency of the pulse, which follow the affusion of cold water on the surface, it may here be inquired how the diminution of the thirst is accounted for. Are we to suppose, that a portion of the liquid is taken up by the absorbents of the surface, as is generally believed to be the case, during immersion in the bath ? I apprehend not. The absorption in the bath, if it occur at all, of which there is no sufficient evidence, has been greatly exaggerated ; and the diminution of thirst, after the cold affusion, is too speedy to be ascribed to such a cause. It admits of a more satisfactory explanation. The morbid stricture on the

the extreme vessels, to which we impute so much, is not confined to the capillaries of the surface, but extends, as far as we have an opportunity of observing, to the capillaries of all the cavities of the body. The sense of thirst is produced by the heat and dryness arising from the morbid stricture of the exhalants in the inner surface of the mouth, fauces, and stomach; and this stricture, being removed by the affusion on the surface of the body, from the sympathy between the surface and these cavities so often alluded to, the healthy action of the exhalants of these parts, as well as of those of the skin, is restored; and their moisture being poured out, the heat and dryness are removed, and the thirst diminished.

This indeed seems to be the way in which thirst is relieved in the first instance, by liquids taken into the stomach itself, where the effect is too sudden to arise from the absorption of any portion of the liquid into the mass of circulating fluids. We cannot indeed perceive this effect on the exhalants of the stomach, but we may reasonably suppose it to take place where we observe the swallowing of drink to be followed by a relaxation of the exhalants on the skin, which must proceed from their sympathy with those of the stomach previously relaxed; and it may illustrate and support this position, that when liquids, swallowed even in large quantities

quantities in the hot stage, do not produce a sensible increase of perspiration on the skin, the relief they afford to the thirst is momentary only. In such cases we presume the stricture on the exhalants of the stomach has not been resolved. The relief from thirst afforded by liquids taken into the stomach is in general however more effectual and complete than from affusion of water on the surface, because, when the stricture on the capillaries is removed, a rapid absorption takes place from the stomach, and the circulating fluids are diluted. But on the surface of the body this absorption is doubtless slow and imperfect, and it will afterwards be shewn, that there is reason to doubt whether it takes place at all. It was remarked that the affusion of tepid water on the surface, though effectual in diminishing heat (page 71), is not followed by the entire cessation of fever, as is often the case after the cold affusion. The cause of this is obvious—the coolness produced by the tepid affusion arises from the increased evaporation, and continues only while this continues. The stimulus given by the tepid affusion is comparatively slight and transient—it does not resolve the spasm on the surface, or on the other cavities of the body, and it does not destroy the morbid catenation. The heat therefore speedily returns, when the remedy is withdrawn.

This

This explanation of the operation of the affusion of cold water seems to me satisfactory. The cases which have been related shew, however, that it is only in the early stages of fever that it produces a solution of the disease. In the after stages, though uniformly advantageous while the morbid heat continues, its effects are not so decisive, the morbid actions having acquired the force of habit, or in other words being carried on by the associations that have been produced.

What then ought to be the indications of practice in fever? To diminish the cold in the cold stage; to moderate the heat in the hot stage, and to resolve the stricture on the extreme vessels by which the morbid heat is retained, and the reaction prolonged; and where the inordinate action of the vascular system continues after these objects have been attained, to support the powers of life till the morbid associations, or habits of action, gradually die away, from the removal of the causes by which they were introduced. In addition to these general indications, it will be essential to secure the proper action of the bowels, and in every case to unload the alimentary canal of its morbid contents, whether these contents have become diseased through the action of general fever, or, as there is reason to believe in some of the fevers

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of the warm climates, be the remote cause by which fever is produced.

If these general observations should excite attention, they will doubtless excite objections also, and perhaps animadversions; but whatever be the fate of his reasonings, the author rests with some confidence on the stability of the facts by which they have been suggested. In the view which he has taken of fever, the supposed intentions of nature are not inquired into; and of course no attempt is made to arrange the symptoms as they arise from the direct agency of the febrile poison, or the exertion of *her* medicating power. Such attempts are as unnecessary as they are difficult. It is not indeed to be denied, that the living system, after being excited into morbid action, passes frequently unassisted, through a succession of symptoms into a state of health, differing in this as in every other respect, from any of the arrangements of inanimate matter. But it is equally true, that it often sinks in the course of this process, sometimes destroyed in fever by the excess of its own re-action, and sometimes by the habits of action or associations produced in the course of the disease.

It is a serious error to suppose that the febrile poison, if so we may call it, being received into

the system, is the principal cause of the symptoms, and that they consist of a struggle of nature to expel it, without which health cannot be restored. It is safer to consider it as an agent that excites the system into fever, which however is carried on, not by the continued presence and agency of this agent, but by the principles which regulate the actions of life. We are not therefore to wait for the senative process by which nature is supposed to separate this *virus*, and to throw it off, watching her motions, and assisting her purposes; but to oppose the fever in every stage of its progress with all our skill, and to bring it to as speedy a termination as is in our power. When we dispel the morbid heat, and reduce morbid re-action in the hot stage of the original paroxysm, by the powerful means of the cold bath effusion, the whole of the febrile symptoms vanish; a sufficient proof, that in this stage of the disease, these symptoms arose from inordinate heat and inordinate action, and not from a poison circulating with the blood.—Hence the safety and the wisdom of decisive measures in the earliest stages, before the strength is materially impaired, or the diseased habits established. Those who practise within the tropics, where fever runs its dreadful course with such rapidity, ought especially to be aware of this truth, with which indeed the most intelligent of their number seem now to be impressed. In those climates

mates, however, the disease ought to be combated, not merely by affusion of cold water on the surface, but by immersion of the patient in the cold bath. In this fever, hours, nay minutes are precious, and as the cold stage is short, the application of this remedy admits little delay. As soon as the second, or hot stage is formed, a judicious and resolute application of cold might supersede all other remedies. But the temperature of the sea, and even of the springs within the tropics, rising generally as high as 77° or 78° of Fahrenheit, affords a stimulus to the sensations from simple affusion, of a weaker kind, and hardly permits us to hope, that in the form of affusion, the cold bath can be followed by those effects in the ardent fever of the torrid zone, which in the milder fever of this climate it so happily produces. Artificial modes of cooling the water may indeed be resorted to, and when this can be effected, the repeated use of the affusion may in all probability extinguish the disease. But on military duty, as well as in most other situations, artificial methods of cooling a sufficient quantity of water it may often be impossible to employ. In such cases, when the physician has ascertained that the heat of his patient has risen to the degree which justifies the practice, let him plunge him fearlessly into the cold bath, or the sea. The duration of immersion must depend on the effects on the pulse, on the sensations, and on

the heat, measured by the thermometer; and a greater degree of coolness will be produced by alternately raising him into the air, where the wind blows over his naked body, and sinking him into the water, than by continued immersion. The utmost care is necessary in a process of this kind, to guard against the effects of fatigue. Various instances might be adduced of the success of this practice in persons who have, in the delirium of fever on shipboard, plunged into the sea. I have received a variety of authentic relations of this nature. These have happened on board of our ships at sea, during the delirium of fever, and chiefly in the warm climates; and in every instance where the patient has been fortunately taken up, recovery has followed.

An accident of this kind occurred about three years ago to Captain S——, of this port, in the Irish channel. He sprang out of his cabin window in the height of delirium, and was upwards of twenty minutes in the water. He was taken up perfectly calm, and speedily recovered.—If it be considered, that in the Irish channel, even in summer, the temperature of the water is seventeen or eighteen degrees colder than in the West Indies, the importance of this fact will be clearly perceived. But while the different modes of applying cold water to the surface are employed, it ought

ought also to be poured into the stomach in large quantities, when the patient's heat will permit it, and the presence of nausea and vomiting is no objection to this practice, if a chilliness of the stomach is not produced.

When we consider the general adoption of cold drink in the ardent fever of the ancients, its success in the Hungarian fever, to which the yellow fever bears such analogy, and the pointed recommendation it has received from Hoffman in bilious vomitings and dysentery, it appears rather extraordinary that it has not hitherto been resorted to in the diseases of the West Indies.

The salutary effects of the cold bath, and of cold drink in fever, strongly recommend the adoption of these remedies in the plague. On the northern and eastern shores of the Mediterranean, where the liberal use of cold drink in fevers prevailed throughout antiquity, and on some parts of which it seems to have been carried to a great extent in the beginning of the present century, there is reason to believe that it has now fallen into disuse. If we give credit to the answers made to the queries of Mr. Howard in regard to the treatment of the plague, the administration of cold drink forms no part of the regular practice in that terrible disease. The Jew physician of Smyrna

na indeed says, " that the Turks, in the violence " of the fever, take handfuls of snow and apply it " over their bodies, and also eat it; and likewise " sometimes throw cold water on their feet." But the learned Hebrew cannot determine whether it is of service, " as these people in other respects " pay no regard to the rules of diet." *Howard on Lazarettos, page* 39. Morandi, physician at Venice, observes, " that some sailors at Constan- " tinople, in the phrenzy of the plague, have " thrown themselves into the sea, and it is said, " that on being taken out, they have recovered." But this happy temerity the regular practitioners do not appear to have imitated, a fact that is further established by Dr. Russel, in his account of the treatment of the plague at Aleppo.*

The administration of cold drink in the plague, as well as the external application of cold water, must however be regulated by the actual state of the patient's heat, and of his sensations of heat, of

* When Mr. Howard was printing his work on *Lazarettos*, at Warrington, in 1788, I communicated to him an account of the successes of our first trials of the affusion of cold water in fever, in the Liverpool Infirmary, and he promised to make more particular inquiries in regard to the empirical use of this remedy, in his ensuing journey to the East, in the earlier part of which, it is well known, his career of benevolence terminated.

of which also we have to lament that we have no accurate information. If it should be found that in this malignant disease, the heat is little or not at all above the natural standard, (as is the case in the advanced stages of the confluent small-pox) our hopes from the exhibition of this powerful remedy would probably be disappointed. But the trial is recommended by every consideration.

CHAP.

# CHAP. XVIII.

*Animal heat—its origin.—Perspiration—its cooling influence.—Mode of operation of other remedies in Fever.—Antimonials—Opium, &c.*

I HAVE elsewhere observed, that if a definition of life were required, it might be most clearly established on that capacity, by which the animal preserves its proper heat under the various degrees of temperature of the medium in which it lives. The more perfect animals possess this power in a superior degree, and to the exercise of their vital functions this is necessary. The inferior animals have it in a lower degree, in a degree however suited to their functions. In vegetables it seems to exist, but in a degree still lower, according to their more limited powers, and humbler destination. As the capacity of preserving nearly an uniform temperature in all the varieties of climate and season, is a criterion of life in the more perfect animals, so among individuals of the same species, the

the degree of this capacity may be considered as a criterion of the strength of the living principle.

There is reason to believe, that while the actual temperature of the human body remains unchanged, its health is not permanently interrupted by the variation in the temperature of the medium that surrounds it; but that a few degrees of increase or diminution of the heat of the system, produces diseases and death. A knowledge therefore of the laws which regulate the vital heat, seems to be the most important branch of physiology.

Modern chemistry assumes to itself the discovery of the origin of animal heat, which is supposed to depend on those changes in the ingesta, which the functions of life are always producing. Among the breathing animals, the principal of these changes is occasioned by the lungs on the atmospheric air which they inhale, and respiration is considered as the principal source of the heat of this class of animals. That the oxygen contained in the atmosphere furnishes the largest portion of the pabulum of life, is now scarcely doubted, respiration being a process in which it passes from its gaseous to its concrete state, giving out a portion of the heat it held in vapour, in the same manner as steam in its conversion into water, gives out a portion

portion of its heat. The heat thus extricated, being conveyed by the circulation to all the parts of the body, is the principal means by which the whole is warmed and animated.*

Some physiologists of our own country, and on the continent, have of late supposed, that a similar process is carried on by the surface of the body, through which also, though in an inferior degree, the oxygen of the atmosphere is inhaled. Experiments are wanting to establish this position, and there

* Respiration is a case of compound elective attraction, in which the oxygen gas is decomposed, and enters into new compounds; a part seems to be absorbed by the general mass of blood circulating through the lungs; a part unites with the hydrogen of the venous blood, and forms water; another part with the carbon of this blood, and forms carbonic acid gas. In the two first cases the language in the text applies strictly, but in the last case the heat extricated does not arise from the oxygen becoming concrete, but from its entering into the composition of a gas which has a less capacity for caloric than itself. The heat disengaged in these different processes unites with the blood, which in losing its hydrogen and carbon (hydro-carbonat) is converted from venous to arterial, and has its capacity for caloric increased. Thus inflammation in the lungs is prevented. But the arterial blood, in the course of circulation, again absorbs by little and little a portion of hydro-carbonat, parting with caloric in the same proportion. Hence the uniformity of temperature throughout the body. This seems the modern theory of respiration, which it must be confessed is not without its difficulties.

there is reason to believe that the principal function of the skin, in the breathing animals at least, is of a different and opposite nature. That an animal possesses to a certain extent the faculty of rendering sensible heat latent, or to speak more philosophically, of reducing caloric from a free to a combined state, in cases in which the stimulus of heat might otherwise overpower the living energy, there is reason to believe, from a variety of experiments and observations. And that this is in part performed by the perspiration from the surface, can scarcely admit of a doubt. The process of perspiration which is continually going on from every part of the body, is in this point of view the converse of that of respiration: as in respiration a gas is constantly converted into a solid or fluid, and thus heat evolved, so in perspiration a fluid is continually converted into a vapour, and thus heat is absorbed. If then we suppose, that while the proportion of oxygen received into the system continues the same, the temperature of the atmosphere is increased, we can understand why our heat is not increased by supposing an increase of perspiration. And if the temperature of the atmosphere remains unchanged, while the oxygen received by the lungs is increased, we can still explain the stability of our heat, by supposing an increase of perspiration. The first of these suppositions is nearly realized, when a warm day comes after cold weather; the

the second is realized, when an increased respiration takes place under exercise—thus perspiration appears to have a principal share in regulating the animal heat, and the chain of life seems connected with the physical world by two links, which the recent discoveries in chemistry enable us to unveil.*

A vessel

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* I have said *nearly* realized, in the beginning of the sentence. Supposing the same bulk of air breathed in warm as in cold weather, the proportion of oxygen will be rather less in the first case, from the diminished density of the air. There is reason to believe also, that an animal contaminates the air more slowly in warm weather, that is, receives a less proportion of the oxygen it inhales, into the system. But these particulars, in the rapid and general views which I offer, are necessarily overlooked.

Dr. Mitchel, of New York, in his paper on the nature of contagion, has asserted, that the actual heat of the animal is always proportioned to the quantity of oxygen inhaled; but a very cursory examination of facts will shew that this is erroneous. By the experiments in Chap. XV. (and various others might be adduced) it will be seen that the living body has the power of preserving its heat nearly unchanged for a considerable length of time in so dense a medium as water 50 degrees colder than the blood. This could not arise from an increased inhalation of oxygen. The phenomenon is partly to be explained from the sudden contraction of the perspiratory organs; but principally, from the powerful impression on the sensations, rousing the vital principle to increased exertion,

and

A vessel filled with water, and exposed to the atmosphere cannot be raised above 212° of Faht. by any quantity of fuel, because, as heat is applied from below, evaporation carries it off from the surface. Hot springs are of the same heat at their fountains throughout the year, probably because the evaporation from the surface of their waters, increases and diminishes with the heat of the atmosphere. In like manner we may suppose the heat of the living body to be kept uniform by the evaporation from its surface increasing or diminishing according to the quantity of heat extricated in the system, or received from the surrounding medium. But the cautious reasoner, aware of the deceptions arising from such analogies, will here very properly inquire—Does the perspiration by the skin, the body remaining at the temperature of health, actually go on more rapidly in warm than in cold weather? Is it greater under exercise than when at rest? Is it more plentiful when oxygen is received in abundance into the system, than when it is imbibed more sparingly? We run little hazard in answering the two first of these questions in the affirmative, but in regard to the last, farther experiments

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and to increased extrication of heat. It is to be observed too, that it was the trunk, and more solid parts of the body that preserved their heat, in the experiments referred to—the extremities became cold.

experiments are requisite to enable us to decide.

The insensible perspiration attracted the notice of physicians very early, and it was a common dogma among the ancients, which has descended with too little examination to our own times, that the whole surface of the body inhales and exhales. The subject has been considered more accurately since the experiments of Sanctorius in the middle of the last century: and the great quantity of matter supposed by him to pass off by this process, pointing out its importance in the animal œconomy, theories of disease, founded on the diminution or obstruction of this discharge, have since his time prevailed universally. The quantity of the matter of perspiration is not easily ascertained, and this uncertainty must continue while the effect of respiration on the weight of the body remains undetermined. It was for some time believed, that the quantity of perspiration from the surface had been exaggerated by Sanctorius; but since a notion has been entertained of the great absorption by the skin, his calculations have been supposed to fall short of the truth. If however we calculate that fifty ounces of water are converted into vapour on the surface of the body in twenty-four hours, this must produce a very considerable absorption of heat, and have a powerful influence on the temperature

temperature of the living system; and however we may distrust the experiments hitherto made, on the quantity of the matter of perspiration, there is no reason to question the points in which they all agree—that this discharge is greater in warm weather than in cold—under exercise than when at rest.*

Important

* Very various calculations have been made of the quantity of matter perspired. The only experiments made in this country on this point, that deserve much notice, are those of Mr. Cruikshanks. He appears to have lost upwards of seven pounds weight in this way in twenty-four hours, when at rest, in an atmosphere of 71°; but under exercise, a much larger quantity. This exceeds the calculation of Sanctorius.

1803. In the report of M. Lavoissier and M. Seguin, on transpiration (perspiration,) published in the last volume of the Memoirs of the Academy of Sciences, printed in 1797, this is stated differently: According to their experiments, the loss of weight by perspiration and respiration which an individual sustains in a moderate heat, and who does not undergo much bodily exercise, varies from 11 to 32 grains in the minute, that is, from one pound eleven ounces and four drachms, to five pounds in twenty-four hours. The medium may be taken at two pounds thirteen ounces in twenty-four hours, of which one pound fourteen ounces passes off by cutaneous perspiration, five ounces five drachms and sixty-two grains by pulmonary perspiration, and three ounces three drachms and ten grains, by the consumption of carbon in respiration. The sum of this loss is however greatly increased by exercise, or an increase in the heat of the atmosphere. It is needless to add, that the experiments of Lavoissier and Seguin

Important as the evaporation from the surface of the body must be in regulating the heat, it is remarkable how little it has hitherto been considered in this point of view. In the year 1755, Dr. Cullen published his discovery of the cold produced by the evaporation of fluids, (*Essays and Observations Physical and Literary*, vol. ii.) a phenomenon long known in Asia, but till then unobserved in Europe, and which has paved the way to so many other discoveries of the modern chemistry. It does not however appear, that Dr. Cullen applied his discovery in explaining the function of perspiration. The effects of evaporation from the surface in cooling the body, was however soon after observed by Dr. Franklin. In a letter, dated *June*, 1758, is the following observation: "During the hot Sunday at Philadelphia, in June 1750, when the thermometer was up at 100° in the shade, I sat in my chamber without exercise, only reading and writing, with no other clothes on than a shirt and pair of long linen drawers, the windows all open, and a brisk wind blowing through the house; the sweat ran off the

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guin carry with them the highest authority, and it is deeply to be lamented that the death of Lavoissiér, and the horrors of the revolution, cut short the experimental inquiry they had undertaken into all the principal functions of the human body, in health and in disease.

" the backs of my hands, and my shirt was often " so wet as to induce me to call for dry ones to " put on. My body however never grew so hot " as the air, or the inanimate bodies immersed in " the air."* It does not indeed appear that Dr. Franklin actually measured his heat, but he makes the inference of his comparative coolness from remembering, that all the bodies about him, even the shirt out of his drawer, felt warm to the touch, and he concludes that he was kept cool " by the " continued sweating, and by the evaporation of " that sweat." Doubtless this reasoning was suggested to Dr. Franklin, and perhaps the whole circumstances recalled to his mind, by the paper of Dr. Cullen, then recently published. When, by the observations of Mess. Du Hamel and Tellet in France, and the experiments of Dr. Fordyce and Sir Charles Blagden in England, a discovery was made of the very extraordinary degrees of heat which the living body can support, philosophical minds naturally endeavoured to account for so singular a phenomenon. That the animal possesses a power of generating cold as well as heat, was the more general opinion; but the conclusiveness of the experiments of our English philosophers to establish this opinion was questioned by many, and among others by my late ingenious friend Dr. Bell, in the paper that forms the first number in

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* *See Franklin's Letters and Papers, p.* 365.

the *Memoirs of the Society of Manchester*. In this paper the effects of the evaporation from the surface in reducing the heat during the experiments in question, is particularly insisted on. In discussing this subject with Dr. Bell, it suggested itself to me, that the principal office of the insensible perspiration might be to regulate the animal heat, and this opinion, which seems so reasonable, has been confirmed by reflection and observation.

But whatever may be the influence of perspiration in cooling the body, it is not perhaps the only process by which this effect is produced. From some experiments in the hot bath, it appears to me that the temperature of the body is with difficulty increased after the sweat begins to flow profusely, and as there can be no evaporation from the skin when the body is immersed in water, and little from the lungs when the air inspired is already loaded with vapour, should my observation be confirmed by future experience, it will perhaps shew, that in the production of sweat itself a degree of heat is absorbed, and thus explain in some degree the reduction of heat that follows profuse perspiration in other cases. It was indeed supposed by Albinus, Haller, and William Hunter, that the sweat, as well as the insensible perspiration, is an exudation of the watery part of the blood through

through the cuticle; but this opinion is contrary to all analogy, and depending on experiments made on the dead body, may be safely rejected, notwithstanding the illustrious names that countenance it. The opinion of Dr. Fordyce and Mr. Cruikshanks seems to be the true one. The matter of perspiration is separated from the blood by the capillary arteries, and thrown out on the surface by organic pores in the cuticle (however difficult to be discovered) connected with the extremities of these arteries; and in this process there is not a separation merely, but a new combination, as in similar instances. In this combination there may be an absorption of heat, and thus the coolness produced by sweating be in part accounted for. The absorption of heat may however take place in various other processes, besides the production and evaporation of perspirable matter; as doubtless its extrication takes place in various other processes besides respiration; and after all, the sudden changes of temperature that take place in certain circumstances, render it probable that the animal possesses powers over its heat not yet understood.*



* Various ingenious arguments have been used by different friends, and correspondents, to shew the improbability of an absorption of heat in the formation of the matter of perspiration;

Leaving this point for future inquiries, the importance of perspiration to the animal œconomy we may safely affirm. By this process the stimulus on the extreme vessels, arising from morbid distension, is diminished or removed, and whatever other methods the system may possess of diminishing its heat, this discharge seems by far the most important, and in certain circumstances essential to that object. In situations where the organs of perspiration are spasmodically constricted, while by the increased momentum of the circulation, heat is preternaturally evolved (as in the commencement of fever) we can easily understand the disorder of the system that ensues.

It is observed, that persons who engage in excessive labour, speedily sink under it, unless they perspire freely, and support their perspiration by drinking some thin and moderately stimulating fluid. This is the case with the reapers in Pensylvania, where the harvest occurs in the hottest season

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tion; this being a supposition which infers, that the matter of perspiration has a greater capacity for heat than the blood from which it is secreted, which is contrary to analogy. I have thrown out the suggestion, but would not be thought to rest any weight upon it. There are phenomena which occur in the sudden heating and cooling of the living body, which I am far from thinking we can account for by any known chemical principles.

season of the year, and who, by means of profuse perspiration, are enabled to work in the sunshine, in which the thermometer rises very often above the heat of the blood.* This is the case with our coal-heavers, who probably lose a fifth or sixth part of their whole weight by this discharge daily, and who supply this waste chiefly by large draughts of porter. In instances of such extreme labour, a sudden stoppage of perspiration from the cessation of exertion, and the accession of cold, is frequently fatal, as has already been explained.

Europeans who go to the West Indies are more healthy in proportion as they perspire freely, especially if they support the discharge by a moderate use of gently stimulating liquids, stopping short of intoxication, and guard against the effects of too excessive an evaporation when their vigour is impaired by fatigue. In the extreme heats of the torrid zone, this indeed is not easy. The highly excitable system of the youthful European is acted on powerfully by the climate. If the orifices of the skin do not pour out a proportional quantity of perspiration, disease must ensue from the direct stimulus of heat; and if the necessary quantity of perspiration takes place, the system is enfeebled by the evacuation, and the extreme vessels losing their tone,

* *See Franklin's Letters and Papers, p. 336.*

tone, continue to transmit the perspirable matter after the heat is reduced as low as its natural standard, or perhaps lower—as occurs in the cold colliquative sweats in the latter stages of fever. In this situation we can easily suppose, that even a slight degree of external cold may become dangerous. The damps and chilliness of night, joined to the increased evaporation produced by the night breeze, must often prove pernicious, especially after exercise and profuse sweating. They rob the surface of its necessary heat, produce a torpor and contraction of the extreme vessels, and the dangerous re-action of the centre, which has already been explained. The proper treatment of the European on his landing in the West Indies, may probably consist in his being placed for a time in some inland and elevated station, where the land being cleared, the air is pure, and less sultry than on the coast; on his avoiding as much as possible any violent exercise; on his guarding against all intoxicating liquids, and whatever increases the heat, and diminishes the strength of the body—taking care however to supply the circulation with liquids proportional to the waste. By such means his system will gradually accustom itself to the climate, the organs of perspiration adapt themselves to the increased heat, and the dangers of sudden change be averted.*

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* 1803. This reasoning would be much strengthened if we

The perspirable matter of the European does not appear to be well fitted to the torrid zone. Consisting

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could rely on an observation of Dr. M'Kitterick Adair, quoted by Dr. Rush in the 5th volume of his Medical Observations and Inquiries, "that the heat of the European on his arrival in "the West Indies, has been observed to be three or four de- "grees higher than that of the natives, or of those accus- "tomed to the climate, to which however it gradually sinks "in the course of time." The experiments made by Dr. Chisholm, in Demerary, to ascertain this point, contradict this assertion, and lead us to suppose that it is one of those inconsiderate observations by which medical science is so often corrupted.

In 12 white persons newly arrived from Great Britain or Ireland, Dr. Chisholm found the mean heat, taken at the arm-pit, 96°.

In 12 white persons whose period of residence within the tropics was from 4 to 20 years, the mean heat was 96°. There was little or no difference when the trial was made on negroes, whether from Africa, or Creoles. These last however seemed to have the greatest heat, the medium of 12 of them being 98°, nor was there any difference in the heat arising from the period of life; infancy and age being of equal temperature.

*See Dr. Chisholm's Essay on the Malignant Pestilential Fever of the West Indies. Vol.* ii. *p.* 468.

If we could implicitly confide in the experiments mentioned by Dr. C. which give twice the proportion of oxygen gas to the atmosphere within the tropics which it possesses in Europe, a different direction would be given to our inquiry respecting the effects of the West India climate on Europeans; but when

Consisting nearly of pure lymph, it is speedily dissipated by evaporation, and without a profuse discharge, the surface cannot be kept moist and cool. On the other hand, the profusion of the discharge debilitates the system, and renders even the cold of evaporation dangerous, especially when this evaporation is increased by an accidental breeze, or the effect of the cold promoted by rest after fatigue. The negro, fitted by nature to the climate, is less accessible to the stimulus of heat, and *his* unctuous sweat less easily dissipated, keeping his skin uniformly moist, sustains a more uniform perspiration, as well as evaporation, and guards the system against the waste and danger of profuse sweating under fatigue, as well as the other consequences already explained. The pungent and stimulating quality of his perspirable matter will promote these salutary effects. In this view of the subject, it may be worthy of investigation, whether the practice of the ancients of using unguents on the skin, still general among the eastern nations, and common among all savages, whose defective clothing obliges them to guard by this means against the viscissitudes of temperature, should not be revived by our countrymen in the West Indies. It has been imagined, that those unguents effectually

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where the result is so unexpected and extraordinary, repeated experiments are necessary to confirm it.

tually obstructed the perspiration; but this notion has been adopted without inquiry, and against probability. These unguents, like the perspirable matter of the negro, may be formed of a consistence, that, while it retards excessive sweating, does not obstruct moderate and necessary perspiration, and being themselves evaporable, they may keep up a coolness that shall diminish the necessity of the natural discharge. In the warm climates the tepid bath seems a necessary adjunct to inunction. It has been for some time used among the French in the West Indies, and is now (as we are informed) beginning to be adopted by the English; and with proper restrictions, it may produce the happiest effects. After exercise under a burning sun, when the strength is wasted by fatigue and perspiration, immersion in the bath heated from 90° to 95° of Fah$^{t}$. will be found most grateful and salutary. It restrains the profuse and colliquative sweating; keeps up the heat of the surface and extremities; and prevents the dangerous re-action of the centre, which a torpor of the extremities so generally produces. It allays the violent action of the heart and arteries, and sooths the system of sensation. On leaving the bath, friction, and inunction of the surface prevent the chill of evaporation, and while they ensure the safety of bathing, they prolong the pleasure of this classic gratification. The refinement of modern manners has introduced linen teguments

ments next the body, and abolished inunctions of the surface. Hence the warm bath has become less safe, as opening the pores of the skin, and exposing the naked body to the chills of evaporation; and thus through the greater part of Europe it no longer forms a part of the regimen of health. In the east it still prevails. If it should be revived among our countrymen in the west, they must conjoin with it the inunctions of the surface, and the fine flannel teguments next the skin, which accompanied its use among the Greeks and Romans. In recommending a trial of unguents to the practitioners of the torrid zone, I may shelter myself under the authority of the celebrated Lord Verulam, unquestionably the first physician, as well as the first philosopher and statesman of the century in which he lived. "*Inunctio ex oleo, et* "*hyeme confert ad sanitatem, per exclusionem* "*frigoris, et æstate, ad detinendos spiritus, et* "*prohibendam exolutionem eorum, et arcendam* "*vim aeris, quæ tunc maxime est prædatoria.* "*Antè omnia igitur usum olei vel olivarum vel* "*amygdali dulcis, ad cutem ab extra unguendum,* "*ad longævitatem conducere existimamus.*"*

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* *See Fran. Bacon. Opera fol. Francofurti ad Moenum*, 1665, *p.* 536, 537, 538, where the practice of various nations respecting unguents is considered at some length, and the cautions requisite in adopting this practice, lest the body should be overheated, &c. are very ingeniously detailed.

The operation of unguents on the surface of the body presents indeed a subject for important and original observation. The effects of the warm and tepid bath, though more investigated, are scarcely better understood, for perhaps there is no part of medicine on which so much has been written, and so little been decided. These subjects are connected together, and might form, with great advantage, part of an experimental inquiry into the laws of animal heat. The commonly received opinion, that the warm bath relaxes and enfeebles the system, must, I apprehend, be admitted with many restrictions. Immersed in water, or in air, heated to the degree that quickens the circulation, we are doubtless speedily enfeebled. But by a heat short of this effect, it may be disputed whether debility is ever produced. The degree to which the bath must be heated in order to quicken the circulation, approaches nearly to that of the blood. In my own case I have found the pulse become more frequent at 96° when the stomach was empty, but at 94° after dinner; and the practice, said to have been adopted by the ancients, of going into the bath to relieve themselves from the oppression of an overloaded stomach, would appear to me to have been attended with some hazard, and with great inconvenience. It does not indeed appear how the desired effect was obtained, except through the means of profuse sweating. In future

experiments

experiments respecting the effects of the warm bath, it will be of importance to observe the heat at which the sensible perspiration begins to flow; which probably varies a good deal in different constitutions, and which, as it varies, will materially affect the results. It will be important also to notice the state of the stomach as to fullness; the condition of the pulse; the previous degree of exercise; and the actual heat of the surface; all of which, according to my observation, will influence the experiments.

In all inflammatory diseases, it is of importance not to use the bath heated to the degree that materially quickens the circulation; where this is not attended to, the symptoms are heightened, unless indeed a speedy and profuse perspiration ensues. In the degrees in which it does not quicken the circulation, the warm bath is soothing and sedative, especially when the immersion is prolonged; and it is the temperature from 90° to 95° that is so singularly restorative after fatigue, though a still lower heat is safe and refreshing, as those who have used the baths of Buxton can testify.—The warm bath is frequently employed to excite a sensible perspiration, which may be prolonged after leaving it; and in this way it may be used with great advantage. Where this object is in view, it is advisable to immerse the patient in the water heated

heated to 94°, or 95°, and very gradually and slowly to increase the heat to 97°, or perhaps 98°, watching its effects. When the sweat begins to appear on the forehead, if the pulse remains calm, and the patient feels at ease, an increase of heat to 99°, or even 100°, may be ventured on with safety, should profuse perspiration be required. But when the bath is heated *in the first instance to* 100°, the stimulus of heat generally produces a feverish circulation, which the subsequent defective perspiration cannot allay. The injurious effects are still greater where the bath is heated at first to 105° or upwards, and sweat, instead of flowing more freely in consequence of the increased temperature, is discharged with greater difficulty, the stimulus of the heat, and the suddenness of its application, inducing a contraction of the extreme vessels of the skin. In this respect, as in most others, the analogy is perfect between the stomach and the surface; for it will uniformly be found, that sweating is excited more easily by draughts of *tepid* liquor, than an equal quantity of the same fluid swallowed as hot as it can be borne.

The sensible perspiration excited in the warm bath does not lower the temperature of the body while immersed, but being prolonged afterwards, it becomes powerfully refrigerant, and is a remedy of great efficacy. These observations however shew

shew the importance of regulating the temperature of the warm bath, by the thermometer, and demonstrate how little dependence can be placed on the observations of those writers who speak of its effects without noting its temperature. A variation of two or three degrees, often impossible to be ascertained by the sensations, will not merely vary the degree, but alter the nature of its effects.

The view given of the nature of fever, and of the functions of the perspiration, seems to afford an obvious explanation of the operation of those antimonial emetics, and sudorifics, by the early exhibition of which fevers are often stopped in their first stages. The direct action of these medicines on the stomach resolves the spasmodic stricture of the capillaries of that organ and of the surface, by which the morbid heat is confined; while the concussion given to the whole system dissolves the morbid catenation, and terminates the disease. These remedies are however uncertain in their beneficial effects, and always highly debilitating. They cannot enter into competition with the affusion of cold water on the surface, but may be employed when the presence of local inflammation precludes the use of that powerful remedy.

These general views cast some light also on that most curious subject, the operation of opium on the

the living principle. In former times it was disputed whether opium was in its nature cold or hot; in our own days it has been disputed whether it is stimulant or sedative. Such abstractions afford exercise for the ingenuity, but in the way in which they have been conducted, they soar above utility and truth. Opium relieves pain, and produces sleep, and these effects being in general concomitant, may be treated of together. But though opium generally produces sleep, yet we are sometimes disappointed in this effect, and especially in those feverish disorders where sleep is of such high importance. About nine years ago, I attended a boy of ten years of age, the son of a friend, dangerously ill of fever. His nights had been sleepless, and the general irritation was very great. Opium had been administered in vain. I determined to give it in larger quantities, and to watch its effects. At ten at night he took forty drops of laudanum; at twelve the same dose was repeated. As I stood by his bed two hours afterwards, he appeared in an imperfect and agitated sleep: his eyes were half open, his face was livid; his lips and skin parched; he was evidently much disturbed, and moaned in his breathing. At this time I perceived that he had tossed the clothes off his feet, which lay naked, and that he grasped the cold bed-post with one of his hands. On examining his hands and feet they felt dry and burning. There was tepid vinegar

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in the room, and I moistened first his hands, and then his feet with it, without awaking him, repeating this practice from time to time: he became more quiet. Thus encouraged, I gradually moistened his legs and thighs in the same manner, and at length his breast and neck, and removing the rest of the bed-clothes, left him covered with a sheet only. In a little while the agitation subsided, and he sunk into a sound, and seemingly peaceful sleep, which continued several hours, the surface of his body being bedewed with a gentle perspiration.

This interesting case pointed out to me clearly, that the sedative effects of opium are often counteracted by the stimulus of heat on the surface and extremities, and that the actual state of the heat of the patient is a circumstance requiring particular attention in the administration of this powerful remedy. The following observations are the result of the inquiry which these reflections suggested.

Opium given in health generally produces sleep, *if the mind be vacant, the stomach empty, and external impressions be excluded.* In this case, its first effect is to increase sensibility, to give a gaiety and liveliness to the imagination, and to diffuse a genial glow over the surface and extremities. The actual heat of the trunk of the body is little if at all increased, but the surface and extremities are brought

brought up to the general temperature, 97° or 98) and a gentle, and has it has been called, a breathing perspiration is diffused over the skin. In this state we sink into those happy slumbers which are ill exchanged for the realities of life. On the approach of sleep, the pulse is generally increased in frequency by four or five pulsations in the minute, and the respiration is slightly irregular, as in the gentler degrees of pleasurable emotion; but as the sleep becomes fixed and profound, the pulse subsides to its lowest and most tranquil state, and the respiration becomes regular, deep, and rather slower than natural. The various circumstances that prevent this favourable operation of opium, even in the state of health, it is not our purpose to consider at present.

When opium is given in fever, if the heat be two or three degrees or upwards above the natural standard, and the skin dry, it seems very generally to increase the heat and restlessness. There are exceptions. If the heat, though preternaturally great, is subsiding, and the skin beginning to soften, though not yet moist, opium very often accelerates the perspiration, and by this means diminishes the heat. In such cases its salutary effects, tranquillity and sleep, generally follow. Thus it will happen, that an anodyne draught given early in the evening, shall occasion increased

heat and agitation, which if deferred till two or three in the morning, would have produced sensible perspiration and repose. In the evening the exacerbation of fever is on the increase, or at its height, which towards morning is subsiding, the difference in the actual heat of the surface being often not less than two degrees or upwards. A dose of opium given in the hot stage of intermittent fever, will often accelerate the sweating stage and shorten the paroxysm, which under similar circumstances in continued fever, has a contrary effect; because in the former case the disposition to perspire is more easily excited. But even in intermittent fever, opium, when given in the hot stage, will be much promoted in its diaphoretic and salutary effects, by moderate draughts of tepid, or if the heat be great, of cold liquids. In continued fever, where the heat is great, and the skin dry, it is proper to lower the temperature of the surface, and if possible to excite sensible perspiration before opium is administered, if we wish to ensure its diaphoretic and soporific effects. But even after opium has been exhibited, when inordinate heat prevents its sedative operation, it will be found safe and salutary to use the tepid or cold affusion; and when the heat is by this means reduced, repose and sleep will follow. Tepid or cold drink will produce, though in a weaker degree, similar benefit. These methods of promoting the diaphoretic effects of opium,

opium, seem more certain and advantageous in fever, than the practice of combining it with ipecacuanha, or the preparations of antimony; but where opium is to be used in inflammatory diseases, or in dysentery, doubtless this last method is to be preferred. These remarks must be considered as applying to opium in its ordinary doses, that is, from half a grain, to two or three grains of the extract, or from ten to sixty drops of the tincture. Perhaps it has been too much the practice of late to give this medicine in large doses, and to overlook its effects in smaller quantities. Experience has convinced me, that considerable effects are produced on the system by a very few drops of the tincture properly administered, and that it is always unwise to employ it in doses larger than necessary to produce the desired effect.

The effects of alcohol (under which term I comprehend the spirituous and vinous liquors) on the system, have a striking resemblance to those of opium. Alcohol is more heating, less diaphoretic, and less soporific. When however *the mind is vacant*, *the stomach empty*, and *external impressions excluded*, alcohol, like opium, has a tendency to induce sleep. As sleep however approaches, the heat of the body rises, especially on the surface and extremities, and stimulating the heart and arteries to increased action, produces a state of agitation with

which sleep is often incompatible. Or if the dose has been such as to benumb the senses, with the first return of sensibility, the drunkard is roused by intolerable heat from his apoplectic slumbers. The degree of heat in the fever of intoxication is doubtless various in various constitutions. I have observed it to rise to 105°, nearly the highest degree of heat in the typhus of this island; and Dr. Alexander in his own case found it as high as 107°.—Under these degrees of heat, the thirst, agitation, and consequent debility, are very considerable, the skin being in general obstinately constricted. The proper treatment in this, as perhaps in all other cases of morbid heat and dry skin, when local inflammation is absent, is to pour large draughts of cold water into the stomach, or to affuse it on the surface. By this practice the heat is reduced, the orifices of the skin opened, sensible perspiration is produced, and one half of the mischief of intoxication is prevented. Similar effects may be produced, though in a weaker degree, by tepid drinks or the tepid affusion.*

The

* 1803. The following narrative, which was communicated to me as illustrative of these observations, I give in the words, and on the respectable authority of Dr. Robertson, late Surgeon-general of the naval hospital in Barbadoes.

"A Gentleman of this island, whose name was Weeks, a

"great

The administration of alcohol in fever requires the same precautions as that of opium. It should not be given when the heat is great, and the skin dry

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" great votary of Bacchus, was in the practice, from fifteen to " twenty years, of plunging into cold water when he rose " from his bottle, and of actually going to sleep in a trough " full of water, with his head supported on a kind of wooden " pillow made for the purpose, above the surface. When he " dined abroad, and had not the conveniences of his own " trough, he used to strip off his coat, waistcoat, and shirt, and " sit exposed in the open air, and in that situation go to sleep, " whether it rained or not. And sometimes he went and " bathed in the nearest adjoining pond, to which he generally " required assistance to be conveyed. The effect of this prac- " tice, was, that instead of experiencing debility, lassitude, head- " ach, and nausea, he found himself on awaking, cheerful and " refreshed, and free from all the effects of intoxication. In " the year 1789, dining one day abroad, he got alternately " drunk and sober three several times before midnight, each " time recovering his sobriety by immersing himself, and " sleeping in cold water; and on awakening returning to " the company. The last time, after supper, he was so im- " moderately intoxicated, that he insisted on his companions " undressing him and carrying him themselves to the pond. " They carried him accordingly in a chair, and set him up " to the chin in water, where he continued upwards of an " hour, a person supporting him. I had this last circumstance " from a Gentleman one of the party, whose veracity may be " entirely depended on.

" At home however he used, as I have already mentioned, " a trough made for the purpose, with a bench in it as a " pillow,

dry and burning: but it may in general be given with safety and advantage, when the heat is nearly of the natural standard; and even when it is somewhat

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" pillow, having been nearly drowned when sleeping in his " pond, from the negro who was appointed to watch him " having himself fallen asleep. In this watery bed he would " sleep, one, two, three, or even more hours, experiencing " always the greatest refreshment. His wife and family, when " they wished him to change his quarters, used to draw out the " plug, and let the water run off, when he awoke, and humor- " ously complained of the loss of his bed-clothes. At length " this expedient began to lose its effect in rousing him, and one " time, he continued to sleep in his empty trough. In conse- " quence of this he was seized with extreme rigours and chills, " followed by a severe attack of rheumatism, which affected him " a long time, and made him desist from the practice in future. " But to the end of his life he was in the habit of sitting, " when intoxicated, with his clothes open, and sometimes " quite naked, exposed to the wind and rain. This extraor- " dinary character died of apoplexy about three weeks ago, " aged sixty three."

*Extract of a letter from Dr. Robertson, dated Barbadoes, 4th March* 1801.

Dr. Robertson had mentioned to me generally the circumstances above related, in conversation, about a year before, and at my request promised to investigate the particulars on his return to Barbadoes. The above is the result. Mr. Weeks was alive many months after Dr. R's. return, and this narrative of his practice may be entirely depended on. It would afford room for many observations. I shall offer one or two.

Should

what above it, if the orifices of the skin are open, and the surface is moist. Opium, as was before remarked, is more sudorific than alcohol, and given in

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Should any one be desirous of following Mr. W's practice, he must recollect, that the cold water of Barbadoes is from 76° to 80°, little inferior in temperature to the Buxton Bath. The superior coolness derived from immersion in water of this temperature, to that in air of the same heat, arises from the greater specific gravity of the water, and its much greater power of conducting heat. An attention to this circumstance might be of the utmost advantage, not merely in diseases of increased temperature, but when the living system, though languishing under excessive heat in the sultry regions of the earth, may be still considered as free from disease. We learn from Dr. Chisholm, who served with the British army in South Carolina and Georgia, in the American war, that in the inland part of these countries, where the atmosphere stagnates during the latter end of summer, and the heat rises to 90° and upwards, the inhabitants often take refuge from the intolerable oppression of the air, by immersing themselves in water. This practice ought to be known to our countrymen in the East Indies. I listened with very painful sensations to the account of the sufferings and death of an officer of great merit and high connexions, who, when recovering from an illness, seems to have sunk under the excessive heat of a stagnating atmosphere in that country. An attempt was made to relieve him, by pouring water on a bundle of wet hay in his chamber; but in a stagnating state of the air, the degree of coolness thus produced would be unequal to the object. Had he been immersed in the water, or had the water been poured over his body, and the air, by artificial means, been made

in that stage of the fever of intoxication in which the skin begins to relax, it often accelerates sensible perspiration, and sometimes occasions it, when it might not otherwise have taken place. In this way I explain a phenomenon which has occurred to others as well as to me, that a moderate dose of opium sometimes gives extraordinary relief to the drunkard, under the sufferings through which he must pass to sanity and strength.

These observations are not offered to the reader as including a complete view of the subject of which they treat. My principal object has been to

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made to blow over him when wet, the relief would in all probability have been effectual. Of these two methods, the first, where the person is feeble, is the safest, and least exhausting. As a cooling process, within the higher degrees of heat to which the atmosphere rises, the last is the most effectual; but the cooling power of evaporation may be carried too far, and requires skilful management. Dr. Chisholm assures me, that, in the West Indies, he has known the heat of the atmosphere suddenly reduced by evaporation, forty degrees.

A consideration of this circumstance will explain why Mr. Weeks suffered when the water was drawn off from his trough, and he lay wet and naked, exposed to the wind. Supposing the water and the air to have been of the same temperature, the greater density of the former was more than compensated by the effects of evaporation; something may also be imputed to the change of medium itself, as explained chap. xv.

to establish the use of a new and powerful remedy in fever, and the general remarks and reasonings which are added, are chiefly with a view to explain its operation and enforce its safety. As however these additional observations are on subjects of great importance, it is my intention at some future period to resume them. In the mean time, it may be objected, that in this rapid sketch, assertions are sometimes given instead of experiments and proofs. In such cases however, it will be found, that the points at issue may be readily brought to the test of experiment; and as general and obscure expressions are every where avoided, it will be easy to resort to this trial; a circumstance which will procure from candid minds a patient hearing, as well as forgiveness for those errors into which I may have fallen.

I have only to add, that the application of cold under any form, where fever is combined with local inflammation, is a subject of much difficulty, and my observations upon it must wait for the elucidations of future experience*. For the same reason

* 1798. Finding that some of my correspondents are disposed to extend the affusion of cold water to symptomatic and inflammatory fevers, and that they are even inclined to think, that in such affections it must act with peculiar advantage, I subjoin to this third edition, a few observations on this point, but rather

reason I have avoided a particular examination of the action of cold on the body in health, whether as

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rather with the view of inculcating caution, than the hope of directing practice.

That the action of cold on the living body is under certain circumstances stimulating, is maintained in *note*, *p.* 73, to which the reader is referred; and this stimulation depends on the sensation by which the application of cold is attended. In fever with local inflammation, there is a morbid degree of heat, for which the application of cold would seem the natural practice. But in such diseases the sensibility to cold (as well as to other powers that stimulate) is also much increased, and hence the difficulty of resorting to it as a remedy. Cold, acting powerfully on the sensations, will in the first instance stimulate the system; and it appears to me probable, that in proportion to this stimulation, is the difficulty of reducing the actual temperature. Hence it may happen, that the injudicious application of cold in inflammatory diseases, shall increase the action of the heart and arteries by the stimulus of sensation, without subtracting *permanently* the stimulus of inordinate heat. This observation extends to measles, catarrh, &c. in which a sense of cold is so easily excited.

To apply cold with success in inflammatory diseases, it seems to me probable, that the sensation which attends it should be as much as possible moderated, and that the application should be so regulated as to ensure the effectual reduction of the temperature of the patient. If this be the case, the sudden and temporary affusion of cold water, so advantageous in typhus, will not be salutary, or indeed safe, in inflammatory fever, in which it appears to me that the patient, if cold is employed, should sink himself

as a strengthening or a debilitating power, an inquiry that ought to be first entered into, in a regular

himself slowly in the cold bath, continuing the immersion for such a duration as the state of his heat, and of the pulsation of his heart and arteries will admit. By the experiments in chap. xv. it appears, that under continued immersion in cold water, the sensation of cold speedily abates, and that the frequency of the pulse is in a most striking and uniform manner reduced. And though the heat in the trunk of the body is wonderfully sustained, yet in the extremities, and over the surface, it is speedily and permanently lessened. It is true these effects may be modified by disease, in such a way as to contradict experiments made on a person in health; and though in pure synocha, the cold bath would probably be advantageous, yet serious objections may present themselves against employing it in pleuritis or peripneumonia; objections which I do not think insurmountable, but which experience neither enables me to invalidate or to strengthen. In phrenitis, gastritis, enteritis, &c. especially in their desperate forms, I think immersion in the cold bath ought to be hazarded.

It may serve to support this reasoning, that the local application of cold to parts inflamed, is regulated by the principles I have laid down. It must not be sudden and temporary, but great in degree, and permanent in duration. Hence the success with which ice, and snow, and the clay-cap, are applied to different parts, for the purpose of preventing or reducing inflammation. In all such cases the sensation of cold speedily subsides, and even though ice be lying on the part affected.

It is well known that the general action of cold may be extended over the system by its application to a part. The use of

gular investigation of the effects of temperature, and its changes, upon life.

CHAP.

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of cold in hemorrhagies is often regulated by this maxim. I have found that hemorrhagy from the lungs may be stopped by immersing the feet in cold water, and perhaps this may be done still more certainly by a permanent application of cold to the penis and scrotum, which part with their heat more easily than any other portion of the surface of the body. I have found that a still more powerful effect was produced in hemoptoe, by immersing the body up to the pubes in cold water, a practice that I can speak of from experience as often safe and efficacious in this disease. But in all these instances, a permanent, as well as powerful application of cold was requisite, and from the analogy between hemorrhagies and inflammations, this may support our reasonings respecting the proper method of employing cold as a remedy in these last diseases.

# CHAP. XIX.

*Is there an Inhalation by the Skin?—Case of P. M. Esq. who died of Inanition.—General Reflections.*

IN speaking of the origin of animal heat, and of the nature and office of perspiration (*p.* 266) it was questioned, whether the inhalation of vapour or water through the skin into the circulating fluids, be established by a sufficient induction of facts, and if it occur at all, it was supposed that the quantity inhaled had been much exaggerated. Among the ancients however, the doctrine in question was universally received, and it has been admitted by the most celebrated physiologists, not merely of the past, but of the present times. The facts and arguments in support of it are detailed by Haller in the 5th volume of the *Elementa Physiologiæ;* but on examination, they will

will be found to afford no direct proof of its truth.* Of the great variety of authors quoted on this occasion, no one indeed affirms the actual increase of the weight of the body in the warm bath, Dessault excepted, and this is a point that Haller himself seems never to have subjected to experiment.

The inhalations through the surface in vegetables, in insects, and in certain of the amphybia, are the analogies that give the most direct support to this doctrine, applied to the human species. It is however controverted by many facts, and if it should ultimately be established, it must be reared on a new foundation. When I was at Buxton in 1788, I made an experiment on the effects of bathing on the weight of my body, and after half an hour's immersion, I found it rather diminished than increased. This experiment had I believe been

* The arguments drawn from the appearances that occur in the dead body, may be wholly set aside; as may the instances of certain narcotic vegetables, applied to the skin, (e. g. tobacco and white hellebore) occasioning sickness at the stomach, which are cases of sympathy, or of absorption *after the cuticle had been eroded.* The arguments drawn from the increase of weight in moist air, and from the vast discharge of urine in diabetic cases, are also wholly fallacious. In diabetes we now know there is no absorption by the skin, and the phenomenon in question, which has been much exaggerated, must be accounted for by the action, not of the skin, but of the lungs.

been made before by Dr. Pearson, with the same result; it has since been repeated frequently with great care,* and it is ascertained as a fact, that no increase in the weight of the body is produced by immersion in water, of the heat of 82°. In the year 1790, I had a patient in diabetes, whose cuticle, as is usual in that disease, was in a morbid state; and being desirous of trying how far the inordinate action of the kidneys might be affected by a gentle stimulus applied to the skin, I immersed him in a bath of the temperature of 96°, weighing him before and after immersion. There was no sensible variation in the weight. This experiment has since been made by Dr. Gerard, in another case of diabetes, an account of which is given in the publication of Dr. Rollo;† and as it was repeated a great number of times with the utmost care and accuracy, it may be considered as established, that immersion in the warm bath in diabetes produces no increase of weight. I have made five different experiments of the same kind on myself, varying the heat of the bath from 87° to 95°, but never in any instance found my weight augmented. It may be said however,

* In one case, a sum of money depended on the issue, and a friend of mine submitted to be the subject of the experiment.

† *Rollo on Diabetes Mellitus*, v. ii. *p.* 72.

ever (and it has been said), that though in diabetes, where the epidermis is diseased, no liquid is inhaled; and though in health, where the vessels are full, no absorption may take place, yet that where the body is wasting from a want of the proper food through the stomach, the compensating powers of the living system may be exerted to supply the defect, and to excite an inhalation by the absorbents under the cuticle, or through those pores on the surface, by which exhalation is usually performed.

The following case will throw some light on this point. I give it however not merely with this view, but as containing other particulars that appear to me curious and interesting.

In August, 1795, a gentleman of Yorkshire, aged 66, applied for my assistance in a dysphagia, (obstructed deglutition) with which he had been for ten or twelve months afflicted. At first the complaint was slight, and occurred only when he attempted to swallow dry and hard substances; it afterwards extended to solids of every kind; and at the time I first saw him, though he was still able to pass down liquids, the quantity he could swallow was not sufficient for his nutrition, and he was considerably reduced. On the introduction of a bougee into the gullet, it passed about two inches easily, but then met with an obstruction, which by a moderate

a moderate pressure was overcome. It then passed easily seven or eight inches more, but experienced a firm resistance in the lower part of the tube, towards its termination in the cardia, which the skill and patience of Mr. Park could not surmount, and which finally baffled every resource of his art. Though we had not an opportunity of examining the œsophagus after death, yet the nature of this obstruction was evident. It doubtless proceeded from a schirrous tumour, gradually increasing, which at first diminished the passage, and in the end obstructed it wholly; and it existed in that part of the tube, which, from its suddenly narrowing, is, as Dr. Hunter has remarked, most exposed to the causes, and most frequently the seat of this disease. Mr. M. never had any considerable pain from the pressure of the bougee on the tumour, and though he expectorated (if the expression may be used) much mucus, there never was any purulence discharged, or any reason to believe that the tumour had ulcerated.

The last time Mr. M. swallowed in any quantity, was on the 17th of October. On the evening of that day a sudden increase of the obstruction came on, and from this time forth he was able to swallow only a table spoonful of liquid at a time, and with long intervals. It was with difficulty that he got down seven or eight spoonfuls of strong

soup in the day, and this quantity gradually diminished. On the first of November, the passage seemed wholly obstructed. As he wasted rapidly, we were obliged, from the 18th of October, to employ the subsidiary modes of nutrition, and he went into the tepid milk and water bath, and had a nutritive clyster administered once a day.

As Mr. M. himself was far from despairing of his recovery to the last, the affectionate friends around him did not wish him to be informed of his real situation. He continued therefore to swallow two or three table spoonfuls of milk daily, till about the 15th of November, on the supposition that some might pass; but though it rested in the œsophagus some time, it was constantly ejected at last; and being fatigued by this expectoration, which disturbed his rest, and by which he perceived that the whole of the milk was thrown up, for the last twenty days of his life he made no attempt to swallow, but contented himself with washing his mouth and fauces with wine and water.

Mr. M.'s friends, though they could not but foresee the issue of his case after all attempts to remove the obstruction had failed, yet desired that his life might be prolonged to the uttermost. The following plan was therefore adopted with this view, on the last days of October, and it was persevered

severed in with the variations afterwards to be mentioned, till the commencement of the delirium that preceded his death.

Every morning at eight o'clock, he had a clyster, consisting of eight ounces of strong broth, made chiefly of membranous parts of beef (being the most nutritious, according to Dr. Fordyce) into which were rubbed two yolks of an egg, and to which were added 40 drops of laudanum. This was repeated at three in the afternoon, and again at nine in the evening; but in the evening, instead of forty, one hundred and twenty drops of laudanum were added, to compensate the want of an anodyne draught, to which he had been accustomed at the hour of rest. Previously to this however, he was placed up to the neck in a tepid bath, of which one fourth was milk, and the rest water; the whole quantity amounting to twenty-four gallons. The temperature was fixed at 96° to accommodate his sensations, and the time of immersion was gradually prolonged from forty-five minutes to an hour.

The size and repetition of the clysters were determined by our experience of what the rectum would retain; the laudanum being added to increase its retention, as well as to allay irritation, and to stimulate the living energy. In a few days

the retention of the rectum improved, and enabled us to enlarge the clysters to 10 oz. of broth, and three yolks of eggs each; to which, from the 12th of November, 8 oz. of white wine were added; the laudanum being increased to 60 drops for each of the clysters in the day, and to 150, 180, 200, and 250 drops for the evening clyster. Thus the whole of his nutriment for twenty-four hours consisted of 30 oz. of broth, 24 oz. of wine, 9 yolks of eggs, and from 250 to 380 drops of laudanum, by clyster; with what liquid might be supposed to be taken up by the absorbents of the surface in the bath. Mr. M. was very tall, and naturally corpulent. In health he had weighed 240lbs. But imperfect nutrition had gradually reduced him, and at the time of commencing this process, he only weighed 179lbs. On the 20th of November he was reduced to 154lbs.; on the 25th to 149lbs. This was the last time we had an opportunity of weighing him. He seems to have wasted about 5lbs. in four days, and on the second of December, when his delirium commenced, he probably weighed 138 or 139lbs, having lost upwards of 100lbs. of his original weight. He lived till the 6th of December, but from the 2d, the rectum no longer retaining the clysters, they were of course omitted, as well as the bath.

About the 25th and 26th of October, there was

was reason to suppose that Mr. M. would have had a shorter date. He had swallowed a very small portion of nutriment for the preceding eight days, and the plan of nutrition just mentioned, though adopted in part, was not fully established. His tongue and lips were at this time become dry; a blister applied to the sternum, discharged a thick coagulable lymph only; and his urine, reduced to a few ounces in the twenty-four hours, was become extremely high coloured, and almost intolerably pungent. In a few days however this state of the tongue went off; the skin became perfectly soft, and was often covered with a gentle moisture; the urine flowed without pain, and of the natural appearance, the quantity being from 24 oz. to 36 oz. in the twenty-four hours—more than the whole of his loss of weight in the same space of time.

Mr. M. had in general a stool after every fifth or sixth clyster, that is, three or four times a week. The substance of these stools was solid, and nearly of the natural fœtor, and the mass perfectly homogenous. The colour was lighter than natural, as is the case in obstructions of the bile duct; their whole appearance resembling what we find in cases of dissection, in the termination of the Ileum or beginning of the Colon, with the exception of being much less liquid. The concoction of the fœces, if I may so speak, was much more perfect towards

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the end, than at the beginning of this period; at first they looked curdy, but at last they approached to the natural appearance.

The heat, examined by the thermometer, was natural, and nearly uniform from first to last. After the clysters there was generally some flushing of the cheek, and drowsiness, with some increase in the warmth of the extremities; but under the axilla there was seldom any variation, and when it rose there a single degree, it was speedily lowered by a gentle diaphoresis.

The pulse, during the period of the scarcity and pungency of the urine, was disturbed, and more frequent. It afterwards subsided to the natural state, and during the whole of the month of November, was not to be distinguished from that of a person in health. On the morning of the first of December it became small and frequent; and still more frequent, though stronger, after the delirium commenced; the state of the pulse, as well as almost all the other symptoms, from that time forth, very nearly resembling the symptoms of the last stage of fever, when it terminates fatally. During this delirium, a perpetual and indistinct muttering occurred, with great restlessness and agitation; the surface and extremities were sometimes of a burning heat, sometimes clammy and cold. The eyes

lost

lost their common direction, the axis of each being turned towards the nose. In this state however the sensibility of the retina was not impaired, but rather increased, for he screamed out on the light of the window being admitted, to which before he had been accustomed. At this time also the sense of touch seemed more than usually acute, for he appeared disturbed with every accidental breath of air. The delirium, and the derangement of vision commenced nearly together, but we observed the derangement of vision first. On the first of December he complained that he sometimes saw double; but it was not till the succeeding day that any considerable incoherence of mind was observed. The pulse became feeble and irregular on the fourth; the respiration, which had been singularly undisturbed, became laborious; the extremities grew cold; and in ninety-six hours after all means of nutrition, as well as all medicine had been abandoned, he ceased to breathe.

Mr. M. complained very little of hunger. Occasionally he expressed a wish that he could swallow, but not often, nor anxiously; and when questioned on the subject of his appetite, always declared that he had no hunger to occasion any uneasiness. The clysters evidently relieved the sense of hunger, and the opium they contained, seemed to have a principal share in producing this relief.

relief. It occasioned quiet and rest after each clyster, and allayed every kind of desire or appetite.

Neither was Mr. M. much disturbed with thirst. This sensation was indeed troublesome during the first days of his abstinence; but it abated, and, as he declared, was always removed by the tepid bath, in which he had the most grateful sensations.

Mr. M.'s spirits were uncommonly even, and his intellect perfectly sound. He occupied himself a good deal in his private concerns, and as usual interested himself in public affairs. As we discouraged all unnecessary exertion, he spent a great part of his time in bed; but till the last few days of his life, he dressed and undressed himself daily, and walked, not only about his room, but through the house. His nights were quiet; his sleep sound, and apparently refreshing. Towards the end of this period, he said he had very lively dreams, but they were all of a pleasant nature. The last conversation I had with him was on the morning of the second of December. He told me that he had had a very gay evening with two Yorkshire Baronets, whom he named; they had pushed the bottle about freely; many jokes had passed, at the recollection of which he laughed heartily,

heartily, a thing uncommon with him. When I observed that this was a very lively dream, he seemed to endeavour to recollect himself, and for a moment appeared sensible that the scene had passed in sleep. But recurring to the circumstances, he spoke of them again as realities, and I made no further attempt to undeceive him. He said he had been walking out that morning, and was pleased to see the spring so far advanced—he had never felt the air so refreshing. At this time, though his pulse was become rapid, his respiration was natural, and his speech distinct, and without effort. He was extremely weak, and scarcely able to turn himself in bed, to which he had for several days been entirely confined. No man had ever perhaps approached death by steps more easy; and it was earnestly to be wished that his remaining moments might be few. This wish was not gratified. In about an hour afterwards, his delirium became complete, during which his strength evidently increased, and the struggle commenced which has already been described, and which lasted ninety-six hours. He died in his sixty-seventh year.

I have been the more particular in noting this case, because I have not found the methods of supporting life under impaired or obstructed deglutition, sufficiently detailed, nor the death from inanition

inanition described with sufficient accuracy. A knowledge of the first may on many occasions be important; and an acquaintance with the last may possibly throw light on some difficult questions that respect not merely the vital, but the intellectual phenomena.

I have not found any case deserving of credit, in which life was so long preserved without the aid of the stomach, two excepted, one recorded by Ramazzini,* and the other mentioned by Mr. Cruikshanks.† The case described by Ramazzini was that of a nun; and the obstruction was not mechanical, but spasmodic, or as he himself supposes, arising from a paralysis of the œsophagus. She is said to have lived sixty-six days without swallowing, the whole of her nutriment consisting of a single clyster of broth, and two yolks of eggs, administered daily. The dysphagia, which came on suddenly, seems also to have gone suddenly off, and she finally recovered. The case is not given with such minuteness by Ramazzini as to inform us whether any attempts at swallowing were made during the dysphagia. As the obstruction was not mechanical,

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* *Ramazzini, Opera Omnia Genevæ*, 1717, *p.* 173. Many hundreds of fabulous relations of long continued abstinence may be found, some of which are noticed by Haller.

† *The Anatomy of the Absorbing Vessels, London*, 1786, *p.* 101.

mechanical, it is probable that such attempts were made daily; and if this be supposed, a suspicion may be entertained, from the nature of the affection, that more or less nutriment might be swallowed. It is observed by Ramazzini, that the sufferings of his patient were so great on the 30th day, that she was anxious for death, and refused to admit the clyster, which however was administered during syncope, and with immediate and happy effects. In this respect her situation was perfectly contrasted by that of Mr. M., who enjoyed ease and serenity of mind till the last struggle, as has already been mentioned.

The case of Mr. Cruikshanks is not given at length, but alluded to in the following words: "That the surface of the skin absorbs other fluids "which come in contact with it, I have not the "least doubt. A patient of mine, with a stricture "of the œsophagus, received nothing, either so- "lid or liquid, into the stomach for *two months;* "he was exceedingly thirsty, and complained of "making no water. I ordered him the warm "bath for an hour evening and morning *for a* "*month;* his thirst vanished, and he made water "in the same manner as when he used to drink "by the mouth, and the fluid descended readily "into the stomach." It is to be regretted that a case so curious is not given in detail. It does not appear

appear what other means of sustenance Mr. Cruikshanks employed besides the tepid bath, especially during the month of abstinence, in which the bath itself does not seem to have been used. On some future occasion it is to be wished that he may supply these deficiencies, especially as the information I am about to give will shew, that the conclusion he draws from this case, of the absorption of the skin, is fallacious.*

Notwithstanding what I had observed formerly, I fully expected in the case of Mr. M. an increase of weight from the immersion, because he always expressed great comfort from the bath, with abatement of thirst, (which however was never great) and because subsequent to the daily use of it, the urine had flowed more plentifully and become less pungent. I expected it also from the authority of general opinion, and was curious to know the extent of the absorption. I weighed him three different times. The first time he was weighed with his clothes on, before he went into the bath, and naked when he returned from it. Our process was therefore complicated. He appeared to have gained

* 1803.—I hoped to have had a communication from Mr. Cruikshanks on this case, as I applied to him, through a friend, on the subject, but I was unsuccessful. He has since paid the debt of nature. I do not consider his case as of any authority.

gained 30 oz. in weight, which gave him much comfort, and this circumstance prevented me from undeceiving him when I afterwards discovered the error.* The second time he stepped perfectly naked upon Merlin's balance immediately before immersion, and again immediately after it, his body being previously dried. The weights were never moved. The result surprised me: I could not distinguish the slightest variation in the weight of his body, though my beam would have detected a single drachm. I repeated this experiment four days afterwards, and with a similar result.—On both of these occasions the immersion continued an hour, and a constant friction had been kept up nearly the whole of this time on the inner surface of the thighs, with the view of increasing the absorbents.

It is worthy of observation, that there was neither increase nor decrease of weight in the bath; yet Mr. M. was at this time wasting twenty ounces in twenty-four hours, and consequently, if there was no absorption in the bath, it might be supposed that he would have lost 5-6ths of an ounce during the hour of immersion. It is the more remarkable that there was no decrease, because in the bath the skin always softened and the forehead became covered

* I discovered that we had been incorrect before the second experiment.

covered with a gentle moisture, which probably arose from the condensation of the vapour from the bath. At the time, however, I considered this as the matter of perspiration, because, after leaving the bath, and going into a bed previously warmed, he usually fell into a gentle and general diaphoresis, the soother of every irritation, and the harbinger of refreshing sleep. So far the case is tolerably complete. I lament that I cannot give the appearances on dissection, as we were not permitted to examine the body after death.

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The reflections suggested by this case are numerous: I shall present one or two that seem important.

The natural state of the pulse during the first month of Mr. M.'s abstinence, when his strength was daily decaying, demonstrates, that the motion of the heart and arteries gives no certain indication of the strength of the system. Pure debility, within certain limits, does not seem to produce a frequent pulse, nor in all cases a feeble one; but when carried to a certain degree, it generally excites that commotion of the system, in which—first the contractions of the heart, and afterwards of the lungs, become more rapid, which in popular language may be denominated the last struggle of nature,

nature, and which may be considered as indicating the approach of death.* To endeavour to reconcile these phenomena to any of the prevailing theories of vital motion, would be a fruitless attempt. Death has not an uniform aspect, but its *phases* do not appear to me numerous. The concluding scene in the life of Mr. M. may illustrate the form of its approach, as well as the nature of the symptoms in the last stage of fever, and of various other diseases, in which similar symptoms occur.

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* In a recent case (April 1798) where deglutition was obstructed from a different cause, the same methods of supporting life, as in the case of Mr. M. were resorted to. But the tepid bath was soon abandoned, the patient conceiving that it irritated a cough, to which he was subject. In this instance, as in that of Mr. M. the approach of death was gentle and easy. The patient, who was aware of his real situation, met his fate with perfect equanimity, and possessed his understanding to the last. Though the pulse became more frequent two or three days preceding his death, yet there was little or nothing of that struggle described in the case of Mr. M. This gentleman had long been wasting under an incurable disease, and had taken little nutrition for some time previous to the commencement of his abstinence. He lived twenty-three days after he desisted from all attempts at deglutition. He never complained of hunger, and the thirst which he felt occurred at intervals only, and was not very severe. The case was unfavourable to more particular observations; the patient having been long subject to a disease of the system, by which the epiglottis and the surrounding parts were finally destroyed, his symptoms were of a mixed nature.

The natural state of the heat, during the inaction of the stomach, might prove, if it were necessary, how little the vital heat is connected with the functions of digestion, and renders it probable, that the general increase of heat which follows the application of many stimulating bodies to this organ, is not the direct effect of the action of the stomach itself, but of its influence on the heart, or diaphragm, or both. It is asserted by Haller, that in cases similar to that of Mr. M. the heat has been found diminished. The observations made by the thermometer in this case throw a doubt on the assertion, while the great facility with which Mr. M. parted with his heat, concurs with a variety of corresponding facts to support the position, that in those situations, where the generation of heat is natural, the power of retaining it in the system is in proportion to the force of the living principle.

But what shall we conclude respecting the absorption of water by the surface of the body in the warm bath?—Admitting that it did not take place in the case of Mr. M. it may be supposed this did not arise from the impermeability of the cuticle, but from the heat of the bath being too high. It may be added, that the analogy of the vegetable kingdom supports this supposition, since vegetables are found to exhale in warm and dry air during the day,

day, but to absorb largely under the moisture and coolness of night. Considerations of this kind might probably influence some learned physicians in London, who were consulted in this case, to propose that the heat of the bath should be as low as 80°.—It was however raised to 96° for two reasons; the first, that I had found no increase of weight in the Buxton bath at 82°; the second, that as Mr. M. parted with his heat easily, he could not support immersion with comfort in a lower temperature.

It is however more likely to be supposed, that though there was no increase of weight in the case of Mr. M. there might be an absorption in the bath, which was countervailed by the increased exhalation.—The experiments I have detailed do not enable us to decide this point absolutely; but from them, and from various other considerations, I entertain little doubt, that though the exhalants of the skin pierce the epidermis, and come into contact with the external air, the mouths of the absorbents terminate under it, and are covered by it; and that while it remains unirritated and entire, no absorption of solid, liquid, or æriform elastic fluid, takes place on the surface. In the instances that are supposed to favour the contrary opinion, it will be found, that the article absorbed is forced

through the epidermis by mechanical pressure; or that the epidermis has been previously destroyed by injury or disease; or if sound, that the article applied to it is of an acrid nature, which first irritates and erodes this tegument, and then coming into contact with the mouths of the lymphatics under it, is of course absorbed.*

After these observations were committed to the press, my attention was drawn to the valuable memoir

* The absorption of mercury, sulphur, oil of turpentine, and similar substances, rubbed on the skin in the form of unguents, comes under the first of these descriptions; the absorption of the same substances, and in some rare instances of lead from the surface of wounds, and of herpetic, or other eruptions, under the second; the absorption of cantharides, aloes, garlic, &c. under the third. I am not ignorant of the experiments of Dr. Falconer and Mr. Abernethy, nor wanting in due respect to those gentlemen; but my own observations, supported by the experiments of M. Seguin, induce me to reject their conclusions. A probable source of error in the experiments supposed to afford a direct proof of cutaneous absorption, is, that they were made on a small part of the body, e. g. the arm or leg, and not on the whole; though if the mouth and nostrils were left free for respiration, it would be as easy, and much more correct, to make experiments for this purpose, as well as for determining the quantity of exhalation, on the whole surface, as on a part. Dr. Marcard, who wrote expressly on the effects of the bath, has adopted the theory of absorption through the skin, without making any experiments.

moir on this subject read before the Academy of Sciences by M. Séguin, of which some account is given by Fourcroy, in the third volume of "*La Médicine eclairée par les Sciences physiques*, *p.* 232." The conclusions of M. Séguin correspond in every particular with those which I have already laid down, and being founded on a great variety of experiments made for the express purpose of determining on the cutaneous absorption, they appear to me to give his inferences all the solidity of truth. The objection, that though there is no increase of weight in the bath, there may be an absorption, which is countervailed by the exhalation, M. Séguin found a satisfactory mode of invalidating. He dissolved in the water of the bath in which he made his experiments, substances which produce a specific effect when received into the system, by which their inhalation might be ascertained. He employed the oxygenated muriat of mercury in solution, on a number of venereal patients, and while the epidermis was entire, he never perceived a single instance of salivation, or even of amendment of their complaints. But in cases where a considerable portion of epidermis was injured or destroyed, as in the itch, the specific effects of mercury on the system were produced. The experiments of M. Séguin are very numerous, and appear to be devised so as to meet every objection. He concludes from the whole, that

while the epidermis is entire, the absorbents of the surface take up neither water nor air, nor any substance diffused or dissolved in either.

Admitting that the inferences of the friend and coadjutor of Lavoissier are just, how are we to explain the refreshment and abatement of thirst, experienced by Mr. M. in the bath. Referring to what is said in pages 253, &c. on the nature of thirst, we may here shortly observe, that as liquids thrown into the stomach relax the exhalants of the surface, so doubtless this action is reciprocal; and that the exhalants of the surface being relaxed in the warm bath, a relaxation in those of the stomach ensues, by which the sense of thirst is abated. In the cold, as well as in the hot state of intermittent fever, the thirst is great; but it is always lessened, and generally removed, on the accession of the sweating stage, though no liquid should have been swallowed.

Will this explanation, it may be asked, account for the benefit derived in accidents at sea, from immersing the body in water, or applying wet substances to its surface, when no drink can be procured? it will explain it in part perhaps, but not wholly.—In the torrid zone, where the greater part of these instances have occurred, the immersion of the body in water, or the application

cation of wet clothes to the skin, will keep the surface cool, by defending it against the direct rays of the sun, and thus prevent the debility produced by an accelerated circulation; it will also defend it, in whatever climate, from the wasting of evaporation, and thus be a means of prolonging the strength. This reasoning is supported by a fact which may be inferred from the circumstances I have mentioned, and which is established by the experiments of M. Séguin, that the body wastes much less in water than in air.

In the Buxton bath I found my weight very slightly diminished; but in the case of Mr. M. and in all our experiments on diabetic patients, there was neither diminution nor increase in the bath. In the experiments of M. Séguin, there was always some diminution in the bath, of whatever temperature, which he ascribes to the pulmonary perspiration. How is this difference accounted for? Though the cutaneous exhalation may have been stopped in Mr. M. during immersion, yet allowing the perspiration by the lungs to have gone on, this alone, supposing no absorption, ought to have produced a diminution of weight in the bath. To this it may be replied, that though no absorption through the skin takes place in the bath, that by the lungs goes on, while in the heat of 96°. the evaporation from the surface of the

water,

loading with moisture the air inhaled, prevents its taking up the moisture in the lungs, and thus diminishes the pulmonary perspiration.—Against this source of error it is probable that M. Séguin guarded. It is in this way that the increase of the weight of the body in moist air, which is said sometimes to occur, and which has been supposed to prove a cutaneous absorption, may be accounted for. In moist air the absorption of the lungs from the atmosphere goes on unchanged, while the pulmonary, as well as the cutaneous perspiration, is diminished, and thus the increased heat of the body may be explained. On this point however more accurate experiments are required.

It was observed, that the discharge by urine alone, in the case of Mr. M. exceeded much in weight the waste of his whole body; and it cannot be doubted that the discharge by stool and perspiration exceeded the weight of the clysters. Thus it appears that the egesta exceeded the ingesta, in a proportion much greater than the waste of his body will explain. How, it may be said, is this accounted for, unless by cutaneous absorption? In the same way, it may be answered, by the action of the lungs.

That the oxygen inhaled in respiration from the

the atmosphere, unites in part with the carbon of the blood, to form carbonic acid gas, and in part with the hydrogen, to form water, is generally admitted. In instances of inanition, we may suppose the proportion in which it unites to these different substances to be varied by the compensating powers of life; that the formation of water is greater than usual, and that of carbonic acid gas less. Thus a portion of the oxygen, which in the ordinary course of things *is exhaled in the form of an elastic vapour, being absorbed in the form of water*, will serve to retard the waste of the body, and to furnish the blood with the proportion of lymph, which affords a proper supply to the secretions of urine and perspiration. In the case of Mr. M. there was no appearance of that acrimony of the fluids which might naturally have been expected from the cessation of the supply of chyle, and which Haller describes as the constant effect of inanition.

This theory of the increased formation of water in the lungs has been employed by Dr. Rutherford of Edinburgh, to account for the vast discharge by the bladder in diabetes. It seems also to account for the phenomenon of the egesta exceeding the ingesta, so observed in that disease. It illustrates the doctrine of Séguin, in

in the memoir already quoted, of which Fourcroy gives an account in the following words—*Le diabète ne provient point de l'eau absorbée dans l'air par la peau, mais du reflux de celle qui ne peut pas être enlevée aux poumons par l'air trop chargé d'humidité*—a doctrine which appears to be defective, from not pointing out the source of the increased quantity of water in the lungs. Beautiful as the theory of Dr. Rutherford is, it cannot be considered as established in diabetes, or in inanition (to which I have extended it) till the air expired by patients under these diseases shall be examined. In the mean time I may observe, that the application of it to the case of Mr. M. accords with the usual œconomy of the living principle, which in remedying the defect of particular organs, does not transfer their powers entire to other parts of the system, but modifies or increases the natural action of some other organ, by which that deficiency may be to a certain degree countervailed. Such an exertion of the living principle was evident in the increased powers of the rectum, which have already been described. These vicarious actions of the system are however defective, whether arising spontaneously, or from the assistance of art. No applications of food to the inner surface of the rectum can long supply the absence of food in the stomach, nor can the want of liquid in that

that organ be long countervailed by applications of moisture to the surface, aided by the modified action of the lungs. After a certain time, though immersed in water to the chin, the thirst will prove intolerable without drink, as the shipwreck described in Chap. xv. will testify; and the sufferings of Tantalus,* which the theories of Dr. Marcard led him to deride, will be rated as highly by the naturalist as by the poet.

The opinion given in the preceding pages having

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* Dr. Marcard, in the work quoted *p.* 189, after asserting the great inhalation of water by the skin in the bath, declares, that no naturalist will estimate the sufferings of Tantalus highly. It seems probable, that immersion in tepid water is more effectual in abating thirst, than immersion in cold water.

The account I have given of Dr. Rutherford's theory of diabetes, I derive from the valuable inaugural dissertation of Dr. Marcet of London, presented to me by the ingenious author.

The explanation offered by Dr. Marcet, of the increased heat in diabetes, will be found I believe irrelevant.—In seven cases of this disease which I have examined, the heat previous to the medical treatment was less, not greater than natural, notwithstanding the almost unanimous concurrence of authors in the contrary statement; a proof among many others of the inaccuracy of the accounts we have of temperature in disease.

having occasioned considerable discussion, I think it right to give an abstract from the report of M. Fourcroy, on the Memoir of M. Séguin, not having been able to procure the Memoir itself.

M. Fourcroy begins with mentioning the circumstances which led M. Séguin to doubt the commonly received opinion, respecting cutaneous absorption, and to make new experiments on the subject. The Memoir read by him contains the result. " Nous n'entrerons point," says M. Fourcroy, " dans le détail des nombreuses expé-
" riences qui y sont consignées, et que l'on
" affoibliroit par la rapidité nécessaire dans un
" rapport ; nous nous contenterons de rappeller
" à l'Académie les principaux résultats qu'elles
" ont donnés. Le premier résultat tiré de trente-
" trois expériences faites sur lui-même, c'est que
" le corps n'augmente pas de poids dans le bain,
" qu'il perd moins dans l'eau que dans l'air, et que
" cette perte suit sur-tout la raison de la tempé-
" rature de l'eau du bain ; que la perte de poids
" dans l'eau à dix ou douze degrés (baromètre à
" vingt-huit pouces), est à celle dans l'air comme
" 6, 5 est à 17 ; qu'à quinze à dix-huit degrés de
" température, cette perte dans l'eau est à celle
" dans l'air comme 7, 5 est à 21, 7 ; que dans l'eau
" chaude à vingt-six ou vingt-huit degrés, elle est
" à celle dans l'air comme 13 est à 23. Deux autres
" personnes

" personnes ont offert des résultats différens dans
" ces pertes relatives, mais elles ont toujours moins
" perdu dans l'eau que dans l'air. M. Séguin
" attribue cette perte moindre à ce que la matière
" de la transpiration insensible n'est point exposée
" au contact de l'air qui doit la dissoudre dans l'état
" ordinaire. Il rend raison de la différence de
" ces pertes à diverses températures de l'air de la
" manière suivante. La perte de poids qu'on
" éprouve dans l'eau à dix ou douze degrés, est
" beaucoup plus foible que celle qui a lieu dans
" l'air, parce qu'il n'y a point de transpiration cu-
" tanée ; il n'existe alors que la transpiration pul-
" monaire : celle qui se fait dans l'eau à dix-huit
" degrés est un peu plus foible que la seule trans-
" piration pulmonaire, parce qu'outre qu'il n'y a
" point de transpiration cutanée dans ce cas
" comme dans le précedent, l'air qui entre dans le
" poumon est chargé d'humidité, et ne dissout pas
" toute celle qui se dégage de ce viscère ; enfin,
" la perte de poids qu'on fait dans un bain d'eau
" à vingt-huit degrés est plus considérable que
" celle qui est produite par la seule transpiration
" pulmonaire dans l'air à cette même température,
" parce qu'alors le corps perd, et par cette der-
" nière transpiration, et par la sueur qui sort des
" vaisseaux exhalés, en raison de l'augmentation
" de mouvement du cœur et des artères, qui,
" comme M. Séguin l'a prouvé dans son mémoire

" sur

" sur la transpiration, est la seule cause de la trans-
" piration sensible, ou de la sueur. Mais, malgré
" la différence de ces trois résultats qui dépendent
" de la température de l'eau du bain, il n'est pas
" moins certain qu'il n'y a point augmentation du
" poids du corps par le bain, et qu'il y a seule-
" ment une perte moins forte que dans l'air, dé-
" pendante de l'absence de celui-ci et de la priva-
" tion de sa qualité dissolvante par rapport à la
" matière de la transpiration.

" Les premières expériences dont nous ve-
" nons d'exposer les résultats généraux les plus
" importans, prouvoient bien que le corps n'aug-
" mente pas de poids dans le bain, mais elles ne
" décidoient point encore l'absorption ou la non-
" absorption par la peau, car on pouvoit objecter
" à leur auteur que la perte moindre que dans
" l'air dépendoit de la portion d'eau absorbée par
" les vaisseaux inhalans. M. Séguin a pensé que,
" pour répondre à cette objection, il falloit faire
" baigner des individus dans des dissolutions de
" substances dont les effets sur l'économie animale
" fussent bien tranchans. Il a employé la disso-
" lution de muriate oxigéné de mercure, à des
" doses connues, en pédiluves, sur plusieurs ma-
" lades attaqués de symptômes vénériens, et il a
" constamment observé que lorsque la peau étoit
" bien saine, et l'épiderme bien entier, il ne
" passoit

" passoit pas de sublimé corrosif dans leurs
" humeurs, ils n'éprouvoient aucun des accidens
" dus à ce sel, et aucune amélioration dans leurs
" maladies, tandis que dans le cas où l'épiderme
" étoit affecté et entamé, comme dans la gale, &c.
" ce sel pénétroit le corps, et produisoit alors les
" effets qui en font reconnoître l'existence dans
" l'économie animale.

" Non content de ces expériences faites dans
" un hôpital sur plusieurs individus malades, M.
" Séguin a cru devoir les recommencer sur un
" sujet sain, les suivre avec une scrupuleuse
" exactitude, jusqu'à ce qu'il eût levé tous les
" doutes, et il s'est choisi lui-même pour sujet de
" ces nouvelles tentatives. En tenant à un
" grand nombre de reprises différentes, pendant
" long-temps à chaque fois, une partie de son
" bras plongée dans une dissolution connue de
" sublimé corrosif à différentes températures, et
" disposée dans un manchon de verre recouvert
" de taffetas gommé, de manière à ce qu'il n'y
" eût point d'évaporation sensible, le reste de
" son corps, excepté sa bouche, étant d'ail-
" leurs enfermé dans l'enveloppe imperméable
" de taffetas ciré, afin de pouvoir apprécier la
" transpiration pulmonaire comme il avoit ap-
" précié la perte dans l'air de la partie du
" bras plongée dans la dissolution, M. Séguin est
" parvenu à obtenir des résultats aussi singuliers

" que

" que nouveaux : nous ne rapporterons ici que
" ceux qui ont trait à l'absorption par la peau.
" Quand la dissolution de deux gros de sublimé
" dans dix livres d'eau est à dix et à vingt-huit
" degrés de température, la quantité de ce sel
" dans le bain est très sensiblement la même après
" l'expérience qu'auparavant, et conséquemment
" il n'y en a pas d'absorbé ; mais dans la même
" dissolution à dix-huit degrés, si la presque to-
" talité du bras très-sain, et dont l'épiderme est
" bien entier, y reste plongée, il y a par heure 1,
" 2 grains de sublimé absorbé, quoique l'eau du
" bain qui tenoit cette portion de sublimé en dis-
" solution ne soit point elle-même absorbée. M.
" Séguin tire de ce singulier résultat l'induction
" que ce n'est pas par les vaisseaux lymphatiques
" que se fait cette absorption du sublimé, car ces
" vaisseaux absorberoient bien plus facilement
" l'eau, que cependant n'éprouve pas de diminu-
" tion, mais par les vaisseaux exhalans. L'auteur
" explique ce phénomène d'une manière très-
" ingénieuse : il pense que ces vaisseaux exhalans
" resserrés par des températures basses telles que
" douze, en évacuant des gouttelettes de sueur
" continuelles par un effort plus considérable du
" cœur et des artères produit par la température
" de vingt-huit degrés, et n'absorbant con-
" séquemment aucune parcelle de sublimé dans
" les deux circonstances, se trouvent tellement
" disposés à la température de dix-huit degrés,

" que

" que suffisament dilatés pour que l'eau de la
" dissolution soit en simple contact à leur
" extrémité avec l'humeur transpiratoire, sans être
" repoussée par l'écoulement de cette humeur,
" comme cela a lieu à vingt-huit degrés ; alors
" ce contact sans mouvement permet à l'humeur
" de la transpiration de dissoudre de proche en
" proche une partie du muriate oxigéné de mer-
" cure dissout dans le bain, de la partager avec
" sa première eau de dissolution, de se mettre avec
" elle dans un véritable état d'équilibre, comme
" cela a lieu dans toutes les dissolutions salines
" mêlées avec de l'eau pure. Ainsi, suivant lui,
" une dissolution saline à dix ou à vingt-huit de-
" grés étant mise en contact avec la peau humaine
" bien saine, il n'y a point d'absorption ni de la
" part de l'eau ni de la part du sel ; la même
" dissolution à dix-huit degrés, mise en contact
" avec la peau qui ne transpire point dans l'eau à
" cette température, permet à l'humeur de la
" transpiration de partager le sel de la dissolution
" jusqu'à l'équilibre de saturation, et de le porter
" dans la circulation : cet effet n'a pas lieu lorsque
" les pores des vaisseaux exhalans sont resserrés
" par une température basse ou traversés par les
" courans de sueur qui repoussent la dissolution
" de sel. Les vaisseaux lymphatiques n'enlèvent
" ni solide, ni liquide, ni fluide élastique à la sur-
" face du corps ; les vaisseaux exhalans n'absorbent
" jamais ni solide insoluble, ni liquide, ni gaz,

" parce

" parce qu'ils sont toujours pleins de la liqueur " transpirable. L'absorption qui n'a lieu que " dans une certaine température, et qui tient à " la tendance à l'équilibre entre un liquide non- " saturé et un liquide saturé, est par cela même " très-bornée.

" M. Séguin, en poursuivant la description " de ses expériences, confirme les assertions " précédentes, par l'histoire de plusieurs malades " vénériens, chez lesquels la dissolution de sub- " limé employée en lotion n'a rien fait tant que " l'épiderme n'a point été entamé; d'autres à " qui le sublimé ainsi que le muriate d'ammo- " niaque et de mercure ou le sel *alembroth*, appli- " qués à sec, n'ont produit d'effets que lorsque la " peau s'entamoit par l'âcreté de ces sels; de " quelques-uns qui n'ont éprouvé ni érosion, ni " action conséquemment du muriate de mercure " ou mercure doux appliqué sur la peau. Le " tartrite d'antimoine et de potasse ou le tartre " stibié, appliqué ainsi sur le ventre, a purgé après " avoir produit des boutons dans le lieu de l'ap- " plication; la gomme gutte, scammonée, ap- " pliquées sur le ventre à sec, et recouvertes " comme les matières précédentes d'un emplâtre " agglutinatif à sa circonférence, n'ont fait naître " aucun effet sensible. L'onguent mercuriel " n'agit que par une friction qui fait pénétrer l'ox- " ide de mercure sous l'épiderme, et le met dans

" le

" le cas d'être absorbé; un onguent fait avec le
" sublimé corrosif entame la peau, et devient un
" des moyens les plus prompts et les plus actifs
" de faire pénétrer du mercure très-oxidé dans le
" torrent de la circulation.

" Il n'est pas nécessaire d'entrer ici dans de
" plus grands détails sur les expériences très-nom-
" breuses de M. Séguin, pour faire concevoir les
" résultats généraux qu'elles lui ont fournis, et
" qu'on peut réduire aux suivans: 1°. Les
" vaisseaux absorbans n'absorbent dans aucun cas
" ni l'eau, ni l'air, ni les matières qui y sont mê-
" lées ou dissoutes; 2°. l'épiderme qui les re-
" couvre exactement dans l'état sain, les empêche
" absolument de faire cette fonction, et ils ne l'ex-
" ercent qu'au-dessous de cette croûte. 3°. Les
" matières dissolubles sont peu-à-peu enlevées à
" l'eau qui les dissout par l'humeur de la transpira-
" tion placée à l'extrémité des vaisseaux exhalans,
" lorsque cette humeur ne coule point en torrent
" comme dans la sueur, ou lorsque les vaisseaux
" ne sont point resserrés comme par une tem-
" pérature trop basse; 4°. les matières liquides
" ou fluides élastiques ne sont point admises dans
" les vaisseaux exhalans, toujours pleins de l'hu-
" meur transpiratoire qui y séjourne ou qui y est
" dans un mouvement inverse à celui de l'absorp-
" tion; 5°. les matières caustiques sèches ne sont

" absorbées qu'après avoir détruit et corrodé l'épi-
" derme ; 6°. les matières sèches non-solubles ne
" peuvent passer dans le système lymphatique,
" que lorsque par une friction plus ou moins forte
" on les a fait pénétrer à travers les mailles et les
" pores de l'épiderme jusqu'à l'espace où s'ouv-
" rent les bouches des vaisseaux absorbans.

" A ces énoncés, qui résultent immédiate-
" ment des expériences indiquées, M. Séguin en
" ajoute d'également importans, qui ne sont que
" des conséquences nécessaires des prémieres, et
" dont nous exposerons ici les principaux : 1°.
" les maladies épidémiques se contractent par la
" voie de la respiration, et les miasmes dissous dans
" l'air déposé dans les poumons sont absorbés par
" les vaisseaux absorbans de ces viscères qui, dé-
" pourvus d'épiderme, jouissent d'une force ab-
" sorbante très-entière ; 2°. le diabête ne provient
" point de l'eau absorbée dans l'air par la peau,
" mais du reflux de celle qui ne peut pas être
" enlevée aux poumons par l'air trop chargé
" d'humidité ; 3°. les amas d'eau ou les diverses
" espèces d'hydropisies ne dépendent que de la
" différence d'action entre les vaisseaux absorbans
" et les vaisseaux exhalans ; 4°. l'absorption com-
" mune dans les absorbans par le vuide qui y
" est produit, soit par la diminution de pression,
" soit par celle des stimulus, leur structure val-
vulaire

" vulaire interne, détermine le mouvement des " fluides de leur extrémité vers le systême des " vaisseaux sanguins; 5°. les matières âcres " et stimulantes, en faisant contracter les vais- " seaux, arrêtent l'absorption; l'affinité des sub- " stances à absorber avec les vaisseaux absorbans " détermine également cette fonction; telle est " la différence d'action des matières nourrissantes " et des purgatives, par rapport au systême des " vaisseaux absorbans abdominaux; 6°. enfin " les virus contraires pénètrent par les poumons; " l'épiderme est un rempart qu'ils ne peuvent " franchir dans l'état sain et dans l'intégrité par- " faite de ce tissu, et ils ne peuvent pas être " absorbés par la peau."

*La Médecine éclairée par les Sciences Physiques*, vol. iii. p. 234.

I think it unnecessary to make any observations on this extract, which will convince every candid mind that our conclusions respecting cutaneous absorption have been hasty, if not wholly erroneous.

1803. As the inquiry respecting the absorption of the surface of the body is not less important than curious, and as this incidental notice of it has excited considerable attention, I shall

give some account of the experiments on this subject, published at Philadelphia, in 1800, by Dr. Baptiste Clement Rousseau, of the island of Hispaniola.

The substances employed by Dr. Rousseau, were, spirit of turpentine, musk, garlick, and camphor; and these were selected, because on absorption, they produce an effect on the system which is obvious and easily ascertained. He details his experiments, however, with spirit of turpentine only, those made with the other substances corresponding with them entirely.

To ascertain the effects of spirit of turpentine when absorbed, Dr. Rousseau swallowed a few drops diluted with sugar and water. The effect was as he expected—in a little while his urine had a strong smell of violets. This effect always follows the introduction of spirit of turpentine into the system through whatever channel, and is, as Dr. R. believes, peculiar to that substance. In order to ascertain the uniformity of this effect of the spirit of turpentine, he had it thrown up into his rectum, in the form of a clyster, mixed up with the yoke of an egg, and diluted with tepid water. The clyster was retained, and no doubt absorbed. In an hour, the urine smelt strongly of violets, and the breath was strongly impregnated

ed with the flavour of turpentine. He inhaled from a narrow necked bottle, containing spirit of turpentine, a dozen times, put the bottle away, and went out into the air. An hour afterwards, his breath smelt strongly of turpentine, and his urine had a strong violet flavour.

Desirous of trying how small a degree of inhalation would produce this effect, at another time he made one single full inspiration in the same manner, and put the bottle away. In the course of an hour, his urine was very sensibly affected with the violet flavour, as before.

He poured a phial of spirit of turpentine on a table in a close room, and walked backwards and forwards for half an hour. At the end of this time, on examining his urine, it had the peculiar violet flavour.

The absorbing power of the lungs, and the rapid effect produced by it on the circulating fluids, and even on the secretions, being manifest, in the experiments made by Dr. Rousseau to determine the question respecting the absorption of the surface of the body, great care was used to guard against the action of the lungs.

Having provided a long pipe which communicated

nicated with the external air, he breathed through it, cautiously excluding all communication with the air of the room in which he sat. He then poured out the same quantity of spirit of turpentine on the table as in the foregoing experiment, stripping off his clothes, and exposing the surface of his body to the emanation. He continued in this situation for two hours, but no effect was produced on his urine, either at the end of that time, or for the whole day afterwards.

He immersed his arm in spirit of turpentine for two hours, luting the vessel containing it to the skin, so that no vapour could escape; no effect was produced on his urine or respiration, though in the course of the experiment, the fingers became painful and inflamed, from the irritation of the spirit of turpentine.

He smeared his body all over several times with the spirit of turpentine, guarding against the inhalation of the vapour by the lungs, by breathing the external air through a tube, as before described. He continued this some time. The surface was irritated, and pulse quickened, but no effect was produced on the urine.

These experiments were frequently repeated, and with the same result, whether the spirit of turpentine

turpentine was employed, or camphor, garlick, or musk, as has already been mentioned.

These results have led Dr. Rousseau to doubt of the absorption by the skin, even in cases where friction is employed, as in the inunction of mercurial ointment. In the heat of the surface of the body, he thinks that mercurial vapours may arise from it, and be absorbed by the lungs, thus producing the specific effects of mercury on the system. In support of this supposition, he mentions the case of M. Achard, as given in the *Journal de Physique*, for October, 1782, who having left a dish, containing 20lbs. of mercury, over a furnace that was daily heated, and being himself exposed to the vapours, experienced a salivation at the end of a few days, as did two other persons who had been exposed in the same manner. I am not however disposed to agree with Dr. Rousseau in this opinion. I think with M. Seguin, that the oxyd of mercury is, by the mechanical action of the friction, made to penetrate the epidermis, and to come in contact with the mouths of the absorbents which lie under its surface. The same seems to be the case in the inuction of opiate ointment, by which the specific effects of opium may certainly be produced on the system. I am also of opinion, that there are parts of the epidermis so thin, and of such extreme

extreme sensibility, as to admit of absorption through them, without the aid of friction. I speak of the glans penis, and of the lips. A melancholy proof was afforded me of this circumstance, in the case which I have mentioned (p. 179), where hydrophobia proceeded from the saliva of a rabid dog applied to the lips. I have also been inclined to believe, that in those cases, where the naked body is immersed for a considerable time, in vapour, or liquid, of a nature that stimulates the epidermis, though it may not destroy it—as for instance in the nitric acid bath—that absorption may take place; but the extreme facility with which the lungs absorb, lead me to doubt the inference from the experiments which supported that conclusion. And in regard to the experiments, which have been supposed to prove the absorption of the gases by the skin, I fairly acknowledge that I doubt them entirely. On the whole, the principal function performed by the surface of the body is perspiration or transpiration, and the principal use of this function is to regulate the animal heat. Under this point of view, which has been hitherto almost wholly overlooked by physiologists, perspiration assumes its true character, and becomes an object of the first importance both in health and disease.

CHAP.

# CHAP. XX.

*Population of Liverpool. Prevalence of fever among the poor. Benefit derived from the Manchester House of Recovery in contagious Diseases. History of the establishment of the Liverpool House of Recovery. Extract from the report of the physicians respecting the means of preventing contagious diseases in Liverpool.*

THE Typhus, or low contagious fever, prevails in all large cities and towns to a degree that those are not aware of, who have not turned their attention to the subject, or whose occupations do not lead them to mix with the labouring poor. In Liverpool it has been supposed that this disease is seldom to be met with, and it is certainly true, that the upper classes of the inhabitants are not often subjected to its ravages. In the last twenty years, it has only once spread among the higher classes so as to occasion any general alarm; and when the extent to which it is constantly present among the poor, shall be proved by authentic documents, this circumstance will serve to demonstrate the narrow sphere of the contagion, and to shew how much it is within the limits of human power to lessen

lessen the frequency of the disease. Before we produce these documents, it will be proper to offer a few general observations.

The population of Liverpool, as taken by actual enumeration, amounted on the first day of January, 1790, to 57,961 persons. In the returns made to Parliament under the "Act for "taking an account of the population of Great "Britain," the number had risen in the beginning of 1801, to 80,759; being an increase of 22,798, in a little more than eleven years.* Of the

* The enumeration in 1789 was made by Mr. Makin Simmons, and published in Mr. Gore's Directory in the year 1790, but omitted in the later editions. It occupied him three months, viz, from October 13th, 1789, to January 13th, 1790, and was executed with great care and accuracy. Mr. Simmons gave the population of each street, separately, enumerating also the front houses, back houses or cottages, and the inhabited cellars, with the number of persons in each of these descriptions of habitation. The general result was as follows;

| | | |
|---|---|---|
| Front houses, . . | 6,540, containing | 39,118 Inhabitants |
| Back houses or cottages, | 1,608, containing | 7,955 Inhabitants |
| Inhabited cellars, . . | 1,728, containing | 6,780 Inhabitants |
| The Workhouse and other charities. . | | 1,879 Inhabitants |
| There were besides empty houses, | 717, | |
| Making in all, . . | 10,593 houses, or if the cellars be deducted, 8,865 houses, containing. . . . . | 55,732 resident Inhabitants. |

But

the inhabitants of Liverpool it is ascertained, that about 9,500 live in cellars under ground, and upwards of 9,000 in back houses, which in general have an imperfect ventilation, especially in the new streets on the south side of the town, where a pernicious practice has been introduced, of building houses to be let to labourers, in small confined courts, which have a communication with the street by a narrow aperture, but no passage for the air through them. Among the inhabitants of the cellars, and of these back houses, the typhus is constantly present, and the number of persons under this disease that apply for medical assistance to the charitable institutions, the public will be astonished to hear, exceeds on an average, 3,000 annually.*

The

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But in estimating the population, one twenty-fifth part should be added for sailors, according to the rule laid down by Mr. Rickman, in his "Observations on the Results of the Po-"pulation Act." This will make an addition of 2,229, making in all 57,961 persons.

The Return in 1801 was 11,446 houses inhabited, and 338 empty; in all 11,784 houses, containing 77,653 resident inhabitants. To this add a twenty-fifth for sailors, and the whole number is 80,759 persons; being an increase of 2,919 houses, and 22,798 inhabitants, in eleven years and a few months.

* From the first of January 1787, to the last of December 1796,

The public charitable institutions in Liverpool for the relief of the sick poor, are the Infirmary and the Dispensary. Fevers, by one of the original rules of the Infirmary, are excluded from that hospital, though this rule, as may be seen from the narrative with which this volume commences, has been occasionally departed from. But with a few exceptions, the care of the poor in fever falls on the Dispensary. Of this institution I was for several years one of the physicians, and such of my remarks respecting it as are not supported by authentic documents, are founded on personal observation. Unless in cases where they are removed into the Infirmary, the care of almost all the inhabitants of cellars, when sick, and of a great part of the inhabitants of the back houses or cottages, devolves on the physicians, surgeons, and apothecaries of the Dispensary. This description

1796, inclusive, a space of ten years, it will appear by the table which follows, that 31,243 cases of fever were admitted on the books of the Dispensary alone, which on an average is 3,124 yearly. If it be supposed that some cases may be denominated typhus by mistake, let it be considered, how many cases of this disease do not appear on the books of the Dispensary, though occurring among the poor, being attended by the surgeons and apothecaries of the benefit clubs to which they belong, &c. The list of diseases on the books of the Dispensary has not been kept with equal accuracy since that period, owing to the frequent changes in the apothecaries and secretaries since the death of Mr. Avison.

scription of persons, amounting in all to about 18,500, do not appear to be healthy, the number of cases admitted annually on the books of the Dispensary, on the average of ten years, exceeding 13,000. In the commencement of the year 1780, a register of these cases was begun by Mr. Avison, late apothecary to this charity, and continued by him to the summer of 1797, when he terminated a life of spotless integrity, singular usefulness, and incessant exertion.* From this valuable record I have formed the following table.

GENERAL

* I insert the diseases of September 1790, extracted from the Register, which at the same time will convey a specimen of our most prevailing diseases, and shew that inflammatory and symptomatic fevers are not included under the general title of fever.

*September*, 1790.

| | | | | | |
|---|---|---|---|---|---|
| Febris | 310 | Catarrhus | 24 | Debilitas | 19 |
| Opthalmia | 59 | Dysenteria | 2 | Hydrops | 8 |
| Cynanche | 13 | Cephalagia | 10 | Scrophula | 11 |
| Pneumonia | 28 | Apoplexia | | Rachitis | |
| Hepatitis | | Paralysis | 4 | Syphilis | 24 |
| Gastritis | | Dyspepsia | 21 | Icterus | 1 |
| Phthisis | 19 | Epilepsia | 1 | Dysuria | 5 |
| Rheumatismus | 52 | Asthma | 16 | Vermes | 5 |
| Erysipelas | 8 | Pertussis | 11 | Psora | 142 |
| Variola | | Colica | 24 | Partus Difficilis | 1 |
| Rubeola | 29 | Cholera | 24 | Ustio | 14 |
| Scarlatina | | Diarhœa | 24 | Ulcus | 61 |
| Aphtha | 13 | Hysteria | 4 | Vulnus | 89 |
| Hœmorrhois | 3 | Convulsio | 2 | | |
| Hœmorrhagia | 4 | Insania | 1 | | 1,086 |

# GENERAL TABLE,

*Shewing the number of Patients admitted on the books of the Liverpool Dispensary, from the first of January* 1780, *till the last of December,* 1796, *a space of seventeen years, distinguishing the numbers of each year, and of each month of every year.*

| *Year.* | *Jan.* | *Feb.* | *March.* | *April.* | *May.* | *June.* | *July.* | *August.* | *Septr.* | *October* | *Novr.* | *Decr.* | *Total.* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1780 | 890 | 888 | 1147 | 995 | 827 | 805 | 787 | 672 | 557 | 825 | 730 | 855 | 9978 |
| 1781 | 893 | 755 | 1074 | 1028 | 940 | 846 | 951 | 889 | 981 | 1212 | 1081 | 1099 | 11749 |
| 1782 | 1106 | 1041 | 1150 | 1243 | 811 | 1605 | 686 | 619 | 722 | 736 | 704 | 760 | 11183 |
| 1783 | 975 | 858 | 1199 | 1232 | 1096 | 1035 | 1064 | 1062 | 1050 | 1094 | 992 | 966 | 12623 |
| 1784 | 738 | 915 | 976 | 969 | 968 | 934 | 895 | 877 | 929 | 934 | 1055 | 1009 | 11199 |
| 1785 | 1092 | 1075 | 1124 | 1082 | 992 | 1015 | 856 | 850 | 1005 | 1057 | 882 | 885 | 11915 |
| 1786 | 982 | 760 | 1117 | 996 | 986 | 791 | 816 | 1010 | 930 | 895 | 858 | 959 | 11100 |
| 1787 | 1092 | 881 | 1060 | 1015 | 1041 | 964 | 820 | 821 | 868 | 848 | 958 | 900 | 11268 |
| 1788 | 1022 | 960 | 882 | 1002 | 944 | 903 | 916 | 1115 | 1009 | 927 | 951 | 1140 | 11771 |
| 1789 | 1415 | 1053 | 1044 | 1153 | 1113 | 1070 | 1123 | 930 | 1017 | 908 | 982 | 1095 | 12903 |
| 1790 | 947 | 1052 | 1361 | 1123 | 1152 | 1098 | 1039 | 1001 | 1093 | 1175 | 1162 | 1034 | 13237 |
| 1791 | 1223 | 1126 | 1323 | 1118 | 1176 | 1166 | 1091 | 1112 | 1241 | 1257 | 1274 | 1093 | 14200 |
| 1792 | 1307 | 1302 | 1427 | 1277 | 1159 | 1353 | 1106 | 1013 | 985 | 1342 | 1143 | 1030 | 14444 |
| 1793 | 1088 | 1037 | 1188 | 1228 | 1247 | 1065 | 1077 | 1065 | 1128 | 1335 | 1215 | 1159 | 13832 |
| 1794 | 1051 | 1175 | 1170 | 1138 | 1112 | 1105 | 1066 | 1089 | 1160 | 1022 | 1306 | 1273 | 13667 |
| 1795 | 1298 | 1369 | 2003 | 1171 | 1209 | 1151 | 1266 | 945 | 1064 | 1259 | 1300 | 1200 | 15235 |
| 1796 | 1120 | 1141 | 1230 | 1402 | 1030 | 988 | 948 | 918 | 934 | 1087 | 967 | 1236 | 13001 |
| | 18239 | 17388 | 20475 | 19172 | 17803 | 17894 | 16507 | 15988 | 16673 | 17918 | 17560 | 17693 | 213,305 |

By the foregoing table it appears, that the number of patients admitted under the care of the Dispensary annually, taking the average of the seventeen years preceding 1797, is 12,548. But if the ten years only preceding 1797, be taken, the average number will be found to be 13,355. It appears that the greatest number of patients admitted in any one year up to 1797, was in 1795, amounting to 15,235, exceeding the average of the last ten years by 1880; and that the smallest number admitted for the seven years up to 1797, was in 1796, falling short of that average by 354, and of the number admitted in 1795, by 2,234. When it is considered that the difference between these two years falls chiefly in the winter and spring months; that the winter and spring of 1795 were the coldest, and those of 1796 the mildest, experienced in our climate for a long series of years; these striking facts will corroborate the statements of Dr. Heberden, in proving, that the common opinion, which supposes a mild winter in our island to be unhealthy, and a severe winter to be favourable to health, is, in both particulars the reverse of the truth. It appears also, that though the number of patients in the winter months of 1794-5, greatly exceeded the general average, yet it was in February, and more especially in March, that the excess rose so very high; on this last month the astonishing number of 2,003 patients

patients having been admitted. In the three preceding months the cold had been intense, but in March the weather became warmer, uncertain, and stormy. Hence it appears, that though intense cold is directly unfavourable to the health of our poor, yet that it is in the transition from this intense cold to warmer weather, that their constitutions sustain the severest shock; a position that is still more fully supported by the decisive evidence, that the month of March is the most unhealthy of the year, and next to it the month of April, taking the average of seventeen years together. On the same evidence we may state the succession of the months in respect to their unhealthiness in the following order—*March—April—January—October—May—December—November—February—June—September—July* and *August.** But though on the whole, summer and autumn are more healthy than winter and spring, the difference is perhaps less than might have been expected; and the difference even between the two months that forms the extremes, is not very great. In the seventeen years recorded in the table,

* It appears that June stands in the table higher than May or December, but this arises from the vast number admitted in June 1782, when the influenza was prevalent, and was confined nearly to that month. Allowance being made for this, June will stand as inserted.

table, the average number of patients for each month of the whole is 1045; the average number in the month of March is 1204; in August 940. These observations however are strictly applicable only to a particular description of persons in a particular situation—to the poor of Liverpool—and without the corroboration of other documents, must not be extended to the general mass of society, or to the kingdom at large.

I proceed to give, from the same authentic register, the table of typhus, the more immediate object of our inquiries.

# A TABLE,

*Shewing the number of Fevers admitted on the books of the Liverpool Dispensary, from the first of January 1780, to the last of December, 1796, inclusive; distinguishing the numbers admitted every year, and each month of every year.*

| *Year* | *Jan.* | *Feb.* | *March.* | *April.* | *May.* | *June.* | *July.* | *August.* | *Septr.* | *October* | *Novr.* | *Decr.* | *Total.* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1780 | 150 | 125 | 179 | 173 | 168 | 183 | 191 | 150 | 129 | 186 | 150 | 133 | 1917 |
| 1781 | 130 | 146 | 180 | 200 | 187 | 154 | 157 | 127 | 167 | 234 | 208 | 223 | 2113 |
| 1782 | 268 | 265 | 231 | 292 | 148 | 159 | 120 | 140 | 143 | 182 | 150 | 158 | 2256 |
| 1783 | 210 | 158 | 184 | 207 | 222 | 212 | 286 | 227 | 265 | 316 | 257 | 273 | 2817 |
| 1784 | 170 | 194 | 245 | 247 | 232 | 225 | 270 | 230 | 266 | 247 | 369 | 297 | 2992 |
| 1785 | 285 | 268 | 296 | 294 | 219 | 187 | 173 | 180 | 186 | 250 | 244 | 182 | 2764 |
| 1786 | 191 | 166 | 216 | 244 | 202 | 155 | 159 | 188 | 169 | 211 | 167 | 197 | 2265 |
| 1787 | 256 | 209 | 301 | 234 | 313 | 356 | 255 | 192 | 218 | 234 | 283 | 326 | 3177 |
| 1788 | 236 | 174 | 213 | 255 | 253 | 245 | 271 | 311 | 258 | 341 | 315 | 295 | 3167 |
| 1789 | 319 | 176 | 338 | 323 | 391 | 205 | 184 | 162 | 212 | 214 | 204 | 208 | 2936 |
| 1790 | 176 | 248 | 337 | 294 | 281 | 247 | 343 | 270 | 310 | 340 | 355 | 269 | 3470 |
| 1791 | 253 | 247 | 277 | 230 | 233 | 240 | 266 | 248 | 300 | 344 | 335 | 371 | 3344 |
| 1792 | 359 | 361 | 269 | 278 | 261 | 237 | 236 | 223 | 211 | 330 | 212 | 174 | 3151 |
| 1793 | 174 | 209 | 221 | 259 | 237 | 334 | 199 | 197 | 338 | 305 | 224 | 228 | 2925 |
| 1794 | 157 | 230 | 383 | 280 | 337 | 305 | 291 | 245 | 303 | 290 | 258 | 326 | 3405 |
| 1795 | 152 | 265 | 546 | 204 | 234 | 230 | 248 | 159 | 196 | 239 | 317 | 180 | 2970 |
| 1796 | 197 | 161 | 266 | 242 | 288 | 176 | 203 | 182 | 254 | 329 | 153 | 247 | 2698 |
| | 3683 | 3602 | 4682 | 4256 | 4206 | 3850 | 3852 | 3431 | 3925 | 4592 | 4201 | 4087 | 48,367 |

Thus it appears, that of 213,305 patients admitted in the seventeen years preceding 1797, at the Dispensary, 48,367 have laboured under typhus; nearly a fourth of the whole; an immense proportion! It is curious to observe, that March takes the lead in respect to the prevalence of fever, as well as of diseases in general, and that August, as before, brings up the rear. It appears also, that the month of March 1795, which exceeded every other month in the production of diseases in general, exceeds in a still greater proportion in the production of fever; no less than 546 patients having been admitted in that month under this single disease. The *year* 1795 has not however the same unfortunate pre-eminence here as in the table of general disease, being surpassed in the number of fevers by several others. But the year 1796 enjoys a similar distinction as formerly, having produced fewer fevers than any other of the last ten, and 545 fewer than the average of the whole. Taking the seventeen years in the table, the average number of fevers annually is 2,845, and the average number monthly is 237. The average number of March rises to 275, and of August, sinks to 202. On the whole the prevalence of fever is greater, and the influence of seasons upon it less, than might have been expected.

For the ten years preceding 1797, there were, on an average, 119 patients ill of fever constantly on the books of the Dispensary; or if the seventeen years preceding 1797 be taken, the constant average number will be 109. Of convalescents, unfit for labour, the average number will be nearly as great. Thus, in Liverpool, 240 of the poor may be considered as constantly rendered incapable of earning their subsistence by this single disease, and as the poor seldom lay up any part of their earnings for a season of sickness, the expense of their maintenance must, in one form or other, fall on the public. If we take this as low as 10l. for each, it will amount to 2,400l. annually.*

It would afford me particular pleasure to introduce authentic documents of the success attending the treatment of the typhus by my brethren of the

* These calculations will not be expected to be minutely correct, but I apprehend they will in general be found under the truth. The average number of fevers annually for the last ten years, is 3,124, and the mean duration of each fever is supposed to be fourteen days; $\frac{3124 \times 14}{365} = 119.8$ daily. If the average of the last seventeen years be taken, this will give 2,845 annually, which produces on the same principles 109 daily. I calculate that the period of convalescence after each fever is fourteen days, or upwards, which gives the same number of convalescents as of patients in fever, deducting those who may die, a much smaller proportion than could well be supposed.

the Dispensary, but the valuable register which furnished materials for the preceding tables, does not mark the issue of the particular cases. In general however it may be asserted, that where the assistance of the Dispensary is resorted to in time, even under the disadvantage of being attended in confined and dirty habitations, a great proportion of cases terminate favourably. The general treatment of fever at the Dispensary, consists in giving antimonial emetics in the first instance, and afterwards bark, opium, and wine. Nourishing food is occasionally administered. Ablution with cold water has been used less than could be wished, and indeed it is in general difficult to employ it in the obscure cellars in which this disease is usually found. The fever that prevails among our poor is remarkably uniform: it is the pure typhus, to which the cordial treatment can be applied with safety, and to a great extent. Whereas among the higher classes, fever is often attended with inflammatory symptoms in the first instance, sometimes with pneumonic symptoms through a considerable part of the disease; and in such cases, the indications of practice being contradictory, success is much less certain.

Though the cure of this disease is a principal object of our charitable institutions in Liverpool, it is to be lamented that hitherto little or nothing has been done for its prevention; although it may be

be confidently asserted, that this object, great as it is, is not beyond the reach of human power.— This infection arises from a want of cleanliness, and ventilation; and its influence is promoted by damp, fatigue, sorrow, and hunger. When the subject shall be seriously taken up by the public, the means of securing cleanliness and ventilation, even in the greater part of our subterraneous dwellings, may be clearly pointed out; and though the secondary causes, if I may so call them, seem less capable of being removed, yet much may be done to alleviate them, when a comprehensive inquiry into the condition and wants of the poor, (the most pressing of all objects in the eye of enlightened patriotism) shall engage the attention of the legislature.*

Important

* The want of a diet sufficiently nutritious, is doubtless one of the causes that promote the typhus, and other diseases, among our poor. This does not seem to arise in general from the price of their labour being inadequate to furnish such a diet, but from their ignorance in the most advantageous modes of cookery, and still more, from their indulging in articles that consume their means without adding to their sustenance. In the eighteen hundred cellars in Liverpool, there are many in which animal food is not tasted more than once a week; but there are very few in which tea is not drunk daily; it is often indeed drank twice a day. The money spent on tea is worse than wasted. It is not only diverted to an article that furnishes no nutrition, but to one that debilitates

Important information in regard to the means of preventing the generation and progress of contagion, may be found in the writings of physicians of our own country and neighbourhood; of Dr. Percival and Dr. Ferriar of Manchester, of Dr. Campbell of Lancaster, of Dr. Haygarth of Bath, and Dr. Clark of Newcastle. The persevering exertions of Dr. Percival, Dr. Ferriar, and the other gentlemen of the faculty attending the Manchester Infirmary, have produced an institution in that town, which deserves to be imitated in every large city and town in the kingdom; a house of reception, or as it is justly called, a House of Recovery for the poor, when labouring under fever. Into this asylum, on the first notice of disease, the patient is immediately removed, and proper methods taken to purify his habitation. By these means the contagion is extinguished in its birth, and the patient himself being removed to a situation where all the resources of art can be employed with advantage for

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tates the empty stomach, and incapacitates for labour. Hence the vast number of dyspeptic complaints among our patients at the public charities, which are almost all to be traced to the use of tea or spirits, often indeed assisted by depression of mind. At the Infirmary and Dispensary together, this class of patients exceeds five hundred annually. The great majority are females.

for his benefit, and removed at a period of the disease when these resources are of great avail, is very generally restored in health to his family and to the community. The benefits of this admirable institution in Manchester, no longer rest on the basis of theory; they are established on the immovable foundation of fact and experience. The prevalence of fever in the town is diminished to a degree that has exceeded all rational expectation; and the fears entertained, that the House of Recovery might spread the contagion through the streets in its vicinity, are found to be entirely groundless.*

The

* The following facts are extracted from the Report of the *Board of Health*, dated, *Manchester*, *May 26th*, 1797. Since the establishment of the *House of Recovery*, the whole number of home patients (i. e. patients requiring attendance at their own habitations) is reduced *one half;* but the number of fever cases, in a far greater proportion. In January 1796, 226 cases of fever were admitted at the Infirmary of Manchester, but in January 1797, only 57. Fears had been entertained that the fever might spread from the House of Recovery into the neighbouring buildings, and so through the town, and this had given rise to a considerable opposition. The streets in the immediate vicinity of the House of Recovery, are Silver-street, Portland-street, &c. In the first eight months *after* the establishment of the House of Recovery, the number of cases of fever admitted on the books of the Infirmary from these streets was 25. In eight months of the preceding

The utility of the House of Recovery in Manchester was still more completely ascertained in the course of the summer of 1797.—" Within these " last four months (I copy a paragraph in a letter from Dr. Ferriar in the month of September of that year)

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preceding year, viz. from the 20th of September, 1795, to the 20th of May 1796, the same streets furnished 267 cases of fever. From the 20th of September 1794 to the 20th of May 1795, they furnished 389 cases; and from the 20th of Sept. 1793 to the 20th of May 1794, 400 cases. Facts such as these, require no comment; they have made a forcible impression in Manchester, as might be expected, and the Report I quote contains public testimonies in favour of this excellent Institution from the *Board* of the *Infirmary*, the *Stranger's Friend Society*, and *The Special Board of the Poor*. The total number of fever-cases received into the House of Recovery from its being opened, the 19th of May 1796, to the 31st of May, 1797, is . 371

| | |
|---|---|
| Of these are discharged cured, . . | 324 |
| Dead, . . . . . | 40 |
| Remaining, . . . . . | 7 |
| | 371. |

The benefit of this institution is not to be calculated by the number of persons cured. Every single removal into the House of Recovery, probably prevents on an average two or three cases of the disease. As the institution goes on, it may be confidently predicted, that the proportion of cures will be still greater than here stated, because experience will induce the poor to apply for relief in the early stages of fever, when the power of medicine may be exerted with the greatest

year) we have been threatened with the return of "an epidemic fever, and dangerous cases of ty- "phus have appeared in every quarter of the "town; yet by selecting those patients who were, "from their symptoms and situation, most likely to "communicate infection, and by removing them "into the fever-ward, the weekly list of home "patients has been prevented from increasing "much beyond their usual number. Our House "of Recovery, which contains accommodations "for twenty-eight patients, is sufficient for Man- "chester at present. In the event of a peace, "and the influx of new inhabitants which it "must occasion, it may be necessary to provide "accommodation for a greater number. But I "think fever-wards capable of containing fifty "patients,

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est advantage. I have the authority of Dr. Ferriar for saying, that since the 31st of May to the 25th of September, two persons only have died in the House of Recovery.

1803. The advantages at first derived from the House of Recovery at Manchester, have not continued in an equal degree, owing to the difficulty of enforcing cleanliness and ventilation in the habitations of the poor, difficulties particularly great in Manchester, where such sudden fluctuations occur in the numbers of the lower classes. The importance of the House of Recovery is however universally admitted there, and a new building to contain 100 beds is now projected for that institution.

" patients, with separate rooms for the reception " of cases of *Scarlatina Anginosa*, *Measles*, or " *Small Pox*, would answer every purpose here, " in times of the greatest emergency, even sup- " posing a very considerable increase of popu- " lation."

Till the latter end of the year 1787, there was no place for the reception of fever, or any other contagious disease, in Liverpool. At that time the fever broke out in the Infirmary, described in the sixth page of this volume, and two wards being fitted up for the patients infected, one for each sex, these were afterwards occupied by such cases of fever as occasionally presented themselves. The reception of fever into these wards continued for five years and one month, during which time 232 cases of fever passed through them. The wards were small, and under ground; in every respect inconveniently situated. They held at most eight beds each, but were properly adapted to six beds only. But they admitted of cleanliness and ventilation, and it was there that an opportunity was afforded of employing the affusion of cold water; of remarking its effects; and of establishing the rules that ought to regulate its application.

From the first however, the admission of fever

fever into the Infirmary (which was contrary to one of the original rules of the institution) was objected to by some of the friends of that charity. They allowed the benefit arising from a receptacle for fever, but they contended that the Infirmary was not adapted to that purpose; and that the attempt to render it a hospital for fever, the wards already mentioned being all that could be allotted to that disease, did not afford the relief required, while it obstructed the original purposes of the institution. The want of an asylum for this disease elsewhere overcame these objections. But about the beginning of 1793, a contagious fever having prevailed in the Workhouse, two spacious wards were fitted up in that building by the Parish Committee, for contagious diseases; and an arrangement having been made for the reception of such cases of fever as presented themselves at the Infirmary, into the wards of the Workhouse, from that time the admission of fever into the Infirmary has ceased.

Contagious diseases, and more particularly fevers, have in general been excluded from the hospitals of England, those of London perhaps excepted; but the evidence of our own Infirmary and Workhouse, of the Chester Infirmary, and of various similar institutions, proves, that under proper regulations they may be admitted under the same

same roof with other diseases, without danger of the infection spreading through the building. It is not however to be denied, that the attendance, and the arrangement requisite for patients under fever, render it a matter of convenience and propriety that they should be received into a separate building; and it is a point capable of demonstration, that hospitals for such diseases stand pre-eminent in point of utility over all other hospitals, those for accidents that require the immediate assistance of surgery alone perhaps excepted. The benefit derived from hospitals in other cases consists in removing disease, and is confined to the patient himself; but in cases of contagion, the evils prevented are much greater than those remedied, and the benefit is by this means extended from the patient himself to the circle by which he is surrounded. The establishment of such hospitals was first suggested by tracing the infection which propagates fever, to its origin, and ascertaining the power of ventilation and cleanliness in preventing and in alleviating the disease. The arguments for such hospitals are strengthened by the improved methods of destroying contagion; and if I do not greatly deceive myself, they are still farther strengthened by the success of that mode of practice in fever, which it is the chief object of this publication to explain and to establish. A vigi-

lant

lant exercise of all the means of prevention, and of cure, might indeed in a short period supersede the use of hospitals, by extinguishing the disease; a prospect in which the philanthropist might more safely indulge, if he could calculate with the same confidence on the wisdom as on the power of his species.

The wards for fever at the Workhouse, have in general secured that immense hospital, often containing 1,200 persons, from the spreading of fever, to which, previous to their establishment, it was perpetually liable; and though they are in the very centre of the building, and cannot be entered but through the common stair-case, yet no single instance has occurred of the contagion extending from them into the other parts of the house. They have also relieved the Infirmary from the necessity of admitting those miserable wretches, who, under the influence of fever, (perhaps caught on ship-board) and refused admittance into private lodging, were brought up to that hospital, where, if not received, they might have perished in the streets. But they have not been of sufficient size to answer the purpose of a general House of Reception for the poor, labouring under fever, throughout the town, nor has their power of accommodation been employed to its full extent with

with this view*. The physicians of the Dispensary, who attend the Workhouse, have authority to remove patients on the first appearance of fever, from any part of the Workhouse itself, into these wards; but they have not the same authority to remove them from the town. The delay occasioned by the forms to be passed through in obtaining the admission of these patients has very generally prevented the attempt being made, and sometimes where it has been made and succeeded, has occasioned the relief to come too late.

In consequence of considerations of this kind, a motion was made at the annual vestry, in March 1796, that a set of fever-wards should be constructed, at the expense of the parish, for the general use of the poor, on the plan of the Manchester House of Recovery. Four wards, capable of containing sixty beds, were stated to be sufficient for the town; it was proposed that they should be erected on the elevated and open ground contiguous

* These fever-wards contain eighteen beds very conveniently, and from the 20th of August 1793 to the 31st of August 1797, a space of four years, only 530 cases passed through them. They did not therefore on an average contain more than six fever cases at a time. Of these 530 cases, 51 died, a small proportion, considering that 16 died in 24 hours after admission, and were in *articulo mortis* when received.

contiguous to the Workhouse, and be supported out of the parish rate. And it was asserted, that this would be a plan, not merely of charity, but of œconomy. The measure of establishing fever-wards was unanimously agreed to, and a committee appointed to carry it into effect; but differences of opinion arose as to the execution of the plan, and all proceedings on the subject were suspended for several years. It has however been resumed, and is now nearly completed, in consequence of the circumstances about to be related.

So far as we can trust the bills of mortality published in Liverpool, and so far as we may consider the annual number of deaths as a fair criterion to judge by, the general health of the town does not appear to have been very materially affected at any one time during the last century, either by the prevalence of epidemic diseases, or by any other cause. The number of deaths bears a pretty fair proportion all along to the increasing population.* But in 1800, a sudden increase

* The most striking exception to this remark, appears in the year 1770, when the number of deaths amounted to 1,562, being an increase of nearly 600 above the years which preceded and followed it. I use here and throughout, the general table of christenings, burials, and marriages, and the annual

increase in the proportion of deaths took place, which rose still higher in 1801, the whole number being in the first year 3,080, and in the last 3,768. From the two preceding years, the average annual number of deaths was 2,406, and this is nearly the number of the year following (1802), so that in 1800 the deaths exceeded by 678, and in 1801, by 1,362, the usual number. So extraordinary an increase of mortality, arising as was evident from the prevalence of contagious diseases, excited considerable attention and alarm, and the motion being renewed in the annual vestry for the establishment of a House of Recovery without delay, the measure was unanimously agreed on: a specific levy of nine-pence in the pound being laid on the parishioners of Liverpool, for the express purpose of erecting this building, and providing a new cemetery for the parish poor. A plan for the building was given by the physicians of the Infirmary and Dispensary, and notwithstanding some unexpected delays, the house is



annual tables of the same kind, printed by Nevett, and published and distributed by the sextons, though I am afraid they are often deficient in accuracy. They differ considerably from the General Table published in the *Abstract of the Answers and Returns to the Population Act*, *p.* 149, said to be collected from the Register of the Town of Liverpool, in which the burials for 1770 are stated at 2,020.

now in great forwardness, and will be ready for the reception of patients in a few months. It is a spacious building, standing alone in a field by the Workhouse, from which it is distant about forty yards. It possesses every advantage of structure and situation, and will contain sixty, or on an emergency, ninety beds, of which one third, on a separate floor, are destined for convalescents. It is to be supported out of the poor-rate, under the management of a sub-committee, chosen out of the general parish committee appointed by the annual vestry, and an accurate account of its expenditure is to be kept and published annually. It is not doubted that this Institution will be found a measure not merely of policy and humanity, but of economy also.*

That

* In erecting a separate building, and forming a separate institution for the reception of fevers, the great number of persons requiring admission into this hospital was chiefly considered. For as I have already observed, it cannot be doubted, that in situations where these are less numerous, the reception of fevers may be safely combined under proper regulations with the usual objects of Infirmaries, as the curious, ample, and most important collection of documents on this subject, published by my friend Dr. Clark, of Newcastle, clearly demonstrates.—*See a Collection of Papers intended to promote an Institution for the cure and prevention of infectious diseases in Newcastle.—Newcastle: printed by S. Hodgson*, 1802.

That the Hospital for contagious diseases should have been erected by the Parish of Liverpool, and that its annual expenses are to be defrayed out of the poor-rate, are circumstances rather unusual in the history of such institutions. The application of the poor-rate to such purposes is however not only legal, but prudent and wise—of all the purposes indeed to which it can be applied, this is perhaps the only one which is no less strongly recommended by policy than by humanity.

I have purposely avoided entering into the detail of all the obstructions which this important measure encountered during the series of years in which it was under discussion, though in some points of view, such a detail might have been instructive. To the credit of the town, it was in the end adopted almost unanimously, as it had from the first been unanimously recommended by the gentlemen of the faculty.

The effects of this institution will however be much limited, if measures are not combined with it for removing the causes from which contagious diseases derive their origin among our poor. Some attention has already been paid to this important object. In the beginning of the year 1802, the corporation of Li-

verpool, being about to apply to Parliament for powers to improve the streets and the police of the town, requested the physicians of the Infirmary and Dispensary to suggest to them "such alterations as might contribute to the "health and comfort of the inhabitants," in order, that where necessary, they might include in the bill about to be brought into Parliament, the powers requisite to carry such alterations into effect. The physicians took this request into serious consideration, and presented a report of considerable extent, including a view of the causes of the uncommon sickliness of the two preceding years, and of the measures requisite to prevent its recurrence, and to remove the frequency of contagion in the habitations of the poor. To lessen as much as possible the contamination of the atmosphere, they recommended that lime should be prevented from being burnt within a certain distance of inhabited houses; that soaperies, tan-yards, and other offensive manufactories should in future be prevented from being established in the town; and where now established, and authorized by usage, that they should, whenever practicable, be purchased by the body-corporate, and the space they occupy converted to other purposes. The same recommendation they extended to slaughter-houses, and to all other offensive trades or manufactories. They recommended,

mended, that in all cases where fire-engines, or steam-engines are necessarily employed in the town or its vicinity, the burning of smoke should be enforced, as well as in all other practicable cases where large volumes of smoke are emitted.—They pointed out the necessity of enforcing cleanliness in the streets, to which end an improvement of the pavement was represented to be essential; and they particularly advised a general review of the common sewers, and an improvement of their structure, on the principles of a report on this particular subject, addressed by them to the Mayor and Magistrates, in 1788. They further advised, that effectual provision should be made for draining the grounds within the liberties, and particularly to the north of the town. "Repeated remonstrances (I quote the words of the report) have been made for the last twenty years, on the collections of standing water, including filth of every kind, which are suffered to remain in the district which extends along the termination of the streets from St. Paul's square to Byrom-street, and to which the low fevers, which in the autumnal months especially, infest these streets, are principally to be imputed. These remonstrances have been passed over, on the ground, as we are informed, that the proprietors of the lands will not agree to the plan necessary for draining them. It is not to be endured, that

that the health of the inhabitants should be perpetually endangered from such a cause, and we cannot conceive any subject more proper to be submitted to the wisdom of Parliament.

"But in a more especial manner, we would direct the attention of the gentlemen of the town-council to the habitations of the poor, and in the first place to the inhabited cellars. The vast number of persons that occupy such dwellings, and the impossibility of finding other habitations, forbid us to hope that any recommendation for preventing them from being generally inhabited, could be attended with immediate effect. But a general survey should be made of these subterraneous dwellings, and such means adopted for promoting their salubrity as circumstances require and admit. Many of these cellars are double, and the apartments next the street, having access to the external air only by the narrow and oblique aperture that forms the door, and which is of course shut during the night; and the back apartment having access to light and air only by its communication with the apartment in front; under such circumstances the generation of disease, and especially of contagious fever, is almost a necessary consequence. In all such cases a direct communication should be made with the external air, by opening a window in each apartment,

apartment, and where this is impossible, by introducing a tube, to the external orifice of which, an air-pump, of an easy and simple construction may be applied when necessary, especially in cases of contagion, and the apartments be speedily and effectually ventilated. This would in a very considerable degree facilitate the means of counteracting contagious diseases, and of rendering the administration of medical assistance safe to those whose duty calls them to administer it. But, besides this, these cellars should, in all practicable cases, have a chimney in each apartment, and open into a back yard, where there is a necessary, and a supply of water; and no cellar should be inhabited, the ceiling of which is not three feet at least above the curb-stone round the door way, by which the communication with the external air may be rendered free and direct. Power should also be obtained, to oblige the owners of cellars to white-wash them, at a stated time in every year, and at all other times when they may have been visited by contagion; and likewise to prevent them from allowing these cellars to be inhabited till the plaster of the walls is perfectly dry.

" In the application of these observations to cellars already inhabited, some modification must necessarily be allowed, but in regard to all houses to

to be built in future, powers should be obtained to prevent the cellars from being inhabited at all, except they be constructed according to the plan just pointed out."

The Report here mentions a few places in which, from local circumstances, the cellars are particularly obnoxious, and advises, that as soon as possible, they should be emptied of their inhabitants, and filled up. It proceeds as follows—

"The habitations of the poor in the greater part of the small and narrow courts back from the streets, are equally objectionable in point of health as in the cellars. It is much to be lamented that such a form of building should have grown into general practice. * * *. Powers should be obtained for preventing ground from being occupied with buildings of this description in future, and the proprietors of courts already built, might be compelled to give them the ventilation of a thorough draught of air, which would in general be obtained with little difficulty. If any courts are permitted to be built in future, it should be on condition that they be made of a certain width; that the entrance should not be through an arch-way; that the houses should not be above two stories high; and that the upper end of the court should be kept open

open.—Every court should have two or more necessaries, according to its size, and a plentiful supply of water.

" The Parish are now building a hospital for the reception of contagious diseases. The benefit of this will be greatly increased, if a power is obtained, enabling the magistrates, on the representation of two or more physicians, of the necessity of the measure, to remove the inhabitants of a dwelling declared to be infected inot another habitation, till measures are employed for destroying this infection. For such a desirable object, it would be necessary to have a few dwellings kept for occasional and temporary use, in different parts of the town.

" But after all, it seems necessary to adopt some means for increasing the number, and improving the structure of the dwellings of the poor. It seems highly desirable to determine on a plan, both for the individual houses, and their arrangement as to each other, that may unite healthfulness and convenience, and serve, not only for present use, but for future example. Such a plan, when perfected, would contribute not merely to the healthfulness, but to the beauty and regularity of the town, while, at the same time,

time, it would be of great advantage to the corporation estate. It would be favourable to industry and good morals. In the dwellings of the poor, which at present too frequently exhibit distress and confusion, might be introduced cleanliness and good order, and decency and comfort might be found in situations which too often excite sentiments of pity and disgust.

" To such a change, other regulations would indeed be requisite, and particularly some means of diminishing the temptations to the use of intoxicating liquors, a great and increasing evil, but of a magnitude too serious to admit of its being on the present occasion fully discussed."

Every attention was paid to this memorial by the gentlemen of the Common-Council of the Corporation, and in the draught of the Bill proposed to be brought into Parliament, such clauses were introduced as were necessary to enable them to carry into effect, the various regulations recommended to their notice. But the interests of different individuals being affected, not merely by these regulations, but by a variety of others proposed in the bill, difficulties have arisen in procuring that general assent, so desirable in all such applications to the legislature. The measure

sure is however in progress, and it is not to be doubted, that by the exercise of candour and patience, every obstacle will be removed, and that the public good will triumph over all inferior considerations.

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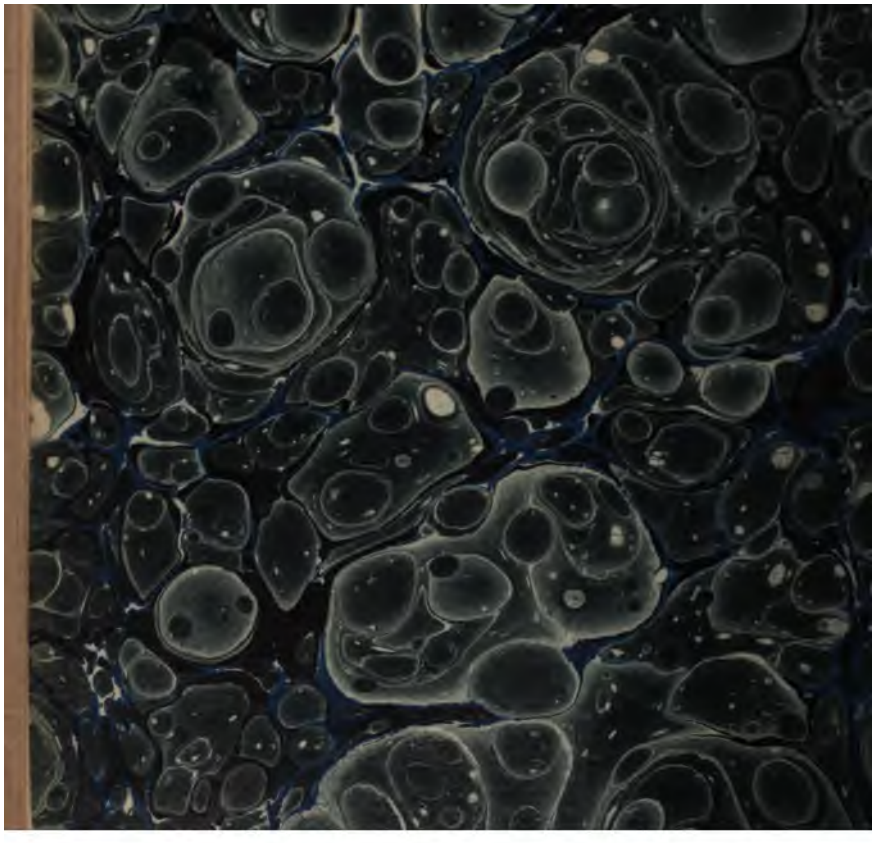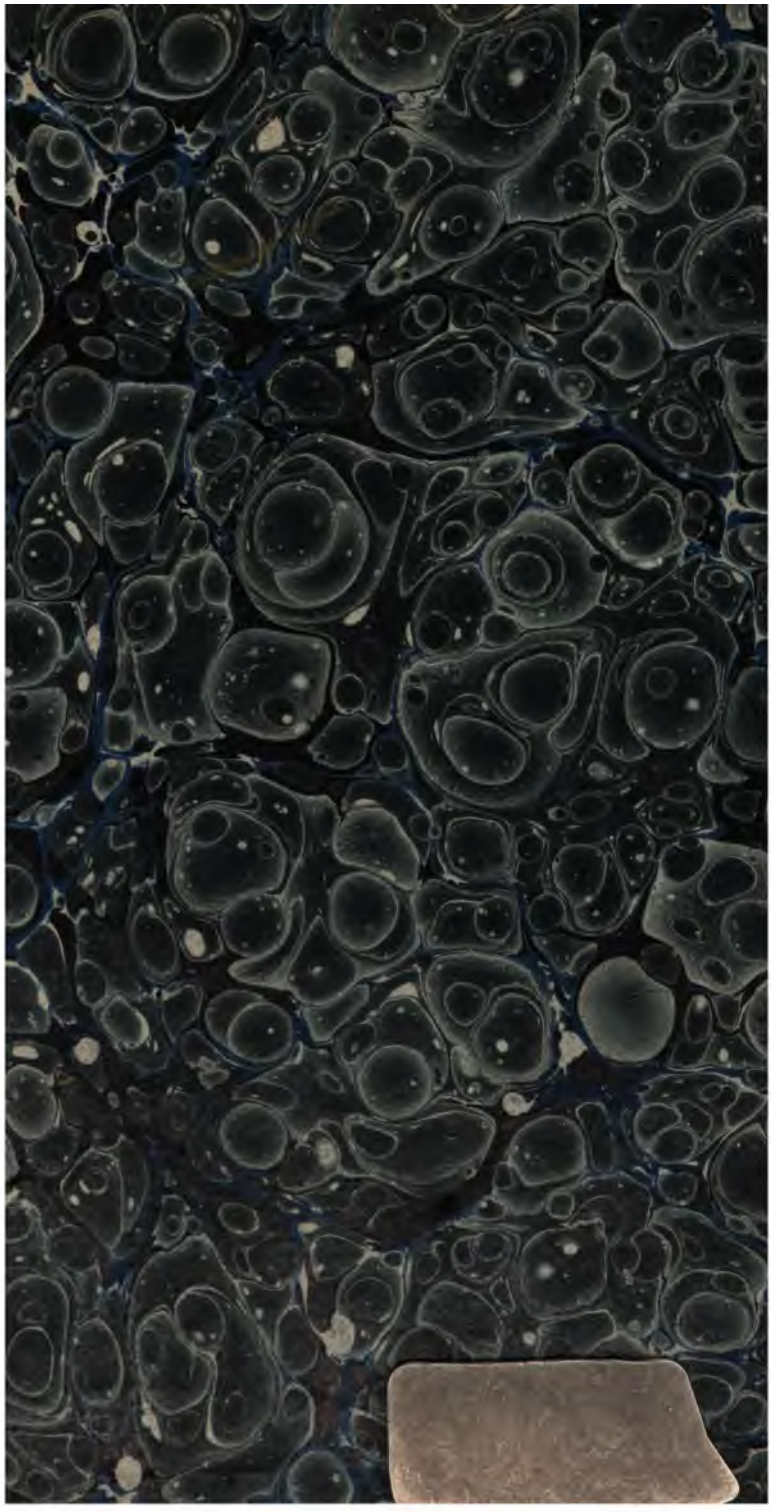
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